SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

FRIDAY, NOVEMBER 11, 1904.

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MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

*DEVELOPMENT OF MORPHOLOGICAL CONCEPTIONS.**

ANY outline of the progress of biology during the century commemorated by this exposition that is compressed within a single address must be either inadequate or must restrict itself to some single point of view. The latter alternative must be the one chosen, not only on account of the vastness of the material, but chiefly that personal experience may give some value to the presentation. In the present address, therefore, certain limitations become necessary, and in this case they are very natural.

In the first place, it would be presumptuous in me to include zoology in any review of progress, for botanists, as a rule, are strictly limited by their material, and have never confounded botany with biology. It is true that such subjects as morphology and physiology are not to be limited by any barrier that may be set up between plants and animals, but it is also true that the material and literature with which one is familiar do not often cross this barrier. At the same time, I think it must be recognized that botany and zoology have been mutually stimulating, every real advance in the one having given an impetus to the other, and that, as a consequence, their progress has been largely along parallel lines. Hence a review of any phase of the progress of the one may serve as an indication of the progress of the other.

* Address delivered at the International Congress of Arts and Science, St. Louis, September, 1904.

In the second place, to outline the progress of biology even from the standpoint of botany is too large a subject to be included in the grasp of any one man in such a way that he can recognize the movements in his own experience. The general botanist no longer exists except in name, and any general survey of botanical activity would have to be a compilation rather than a contribution. With these limitations, it becomes necessary for me to restrict myself largely to such an outlook as is given by plant morphology, and even then to speak only of those conclusions that come naturally to one in contact with the morphology of vascular plants. And yet I believe that a history of the development of the fundamental conceptions of plant morphology may be taken as a fair illustration of what has been going on not only in botany in general, but also in biology.

In the third place, the period included in this survey of plant morphology need not extend beyond the middle of the last century, for at least three reasons: (1) The earlier progress of the science has been outlined by Sachs in his admirable 'History of Botany'; (2) modern morphology finds its beginnings in a very real sense in the work of Hofmeister; and (3) Darwin's theory of natural selection gave the strong evolutionary impulse that it has felt ever since.

My principal theme, therefore, is the development of morphological conceptions, as illustrated by plant morphology.

It would be confusing to introduce the mass of details and the names of investigators suggested by this subject. Nor would there be any advantage in recording the changes of conceptions in reference to the great variety of structures developed by the plant body and in reference to their relation to one another. My purpose is to illustrate the general change of attitude, the shifting of the point of view in reference to plant organs as knowledge has increased. No definite names or dates can be cited, for the movement has been general and gradual, developed out of common experience and proceeding from the background of accumulated knowledge. Disregarding the numerous possible subdivisions, the attitude of mind towards a plant organ during the last half century has presented three distinct phases.

1. THE PHASE OF THE MATURE ORGAN.

At the beginning of the period under consideration, the morphologist concerned himself chiefly with completed organs, and an overshadowing rigid taxonomy compelled the idea of their classification. A few theoretical types of organs had been selected, and all organs were forced by the doctrine of metamorphosis to lie upon this procrustean bed. All parts of vascular plants, for example, were regarded as roots, stems or leaves under various disguises. It does not seem unreasonable to characterize this conception as the arbitrary selection of an ideal type, the natural offspring of the conception of ideal types that prevailed in taxonomy. In other words, morphology was dominated by taxonomy, and morphologists were first and chiefly taxonomists. It is this phase of morphology that must continue to be exploited chiefly by taxonomists, and which still remains in those conservative schools in which instruction lags far behind research. This doctrine of types resulted in the cataloguing of organs just as species were being catalogued, and, although capable of recording material, was incapable of advancing knowledge.

An accompaniment of this mental attitude was the explanation of metamorphoses. It is almost impossible for one age to conceive of the mental condition that was satisfied with the explanations of a previous age. In this case it must be remem-

bered that the earlier botanists were either ecclesiastically trained or not trained at all, and to them it was entirely satisfying to explain all metamorphoses upon teleological grounds. It is a matter of great surprise, however, to note how this point of view is still maintained by some investigators who have abandoned the doctrine of types, and in every other respect are inhaling a modern atmosphere.

One serious result of belief in the doctrine of types was the use of the most complex structures to explain the simpler ones; the reading of complexity into simplicity. For example, the type flower selected was one that had become completely differentiated; in short, a highly organized flower. This was read into all simpler flowers, and was even carried over the boundary of angiosperms and applied among gymnosperms, to the utter confusion of terminology and understanding. Fortunately for the students of cryptogams, a great gulf was thought to be fixed between plants with seeds and those without, and this the flower did not cross.

It is safe to say that this phase of morphology, with its types, and teleology, and simplification of complex structures, is now in its decline.

2. THE PHASE OF THE STRUCTURE OF THE DEVELOPING ORGAN.

This type of morphology has chiefly characterized the period under consideration. Its fundamental conception is evolution; its purpose is to discover phylogeny; and its method is based upon the belief that ontogeny recapitulates phylogeny. As a consequence, there was developed for the first time what may be called a philosophy of the plant kingdom, organizing the details of morphology into one coherent whole about such central facts as alternation of generations and heterospory. Study of the metamorphoses of plant organs was replaced by a study of their development and of 'life-histories,' and the earliest stages of gametophyte and sporophyte and reproductive organs were scrutinized and recorded in the greatest detail in the search for relationships. Shifting its center of gravity from the mature organ to the nascent organ, morphology departed very far from special taxonomy, while at the same time it was laying the solid foundation for general taxonomy. The reversal of old ideas was conspicuous, and much of the old terminology was found to be false in suggestion and almost impossible to shake off. For example, it has been a constant surprise to me to see the persistent use of a sex terminology in connection with flowers by those who must know better, and who must know also that they are helping to perpetuate a radical misconception.

A still more important result of this change of front in the morphological attack was the necessary reversal of the method of interpretation. No longer was the flower of highly organized angiosperms read down into the structures of the lower groups; but from the simplest beginnings structures were traced through increasing complexity and seen to end in the flower, explaining what it is. This meant that evolution had replaced the old idea of types and metamorphosis, and was building facts into a structure rather than cataloguing them. This spirit of modern morphology has not as yet dominated instruction. Its facts are developed in all their detail, abundantly and skilfully, but very seldom do the facts seem to be coordinated. The old spirit of accumulating unrelated material still dominates teaching, and crams the memory without developing permanent tissue.

The detailed developmental study of plants and their organs gave rise to what has been called morphological cytology, but it is an unfortunate differentiation, for cytology merely pushes the search for

structure to the limits of technique. It is becoming more and more clear that every morphologist must also be a cytologist; and certainly every cytologist should be a morphologist; and there is no more reason for differentiation on this basis than on the basis of objectives used.

While fully recognizing the magnificent development of morphological knowledge that has resulted from this point of view, it is interesting to note running all through it much of the rigidity of the older morphology, leavened to a certain extent by the demands of evolution. Certain definite morphological conceptions were established, and organs were as rigidly outlined and defined as under the old regime. For example, there were no more definite morphological conceptions than sporangium, antheridium and archegonium. Unconsciously, perhaps, a type of each was selected, this time from their display in the lower plant groups; and this type was read into the structure of higher groups. The distinctly outlined antheridia and archegonia of bryophytes were compelled to remain just as distinct of definition when they become confused among surrounding tissues in the pteridophytes; and the beautifully distinct sporangium of the leptosporangiates compelled the idea of an imbedded sporangium among the eusporangiates. In other words, the concept included non-essential with essential structures, a distinct wall about a sporangium being just as much a part of the definition as the sporogenous tissue, and its presence compelled even in the absence of any occasion for it. It can hardly be doubted that this was a heritage of habit from the older morphology, for it is in a sense a continuation of the conception of types. The recent morphologist who traces a sporangium wall into an anther is the same in spirit as the older morphologist who saw in the stamen a transformed leaf.

Associated with this rigidity of conception as to structure was the idea of predestination, and search was made for the cell or cell-group that was foreordained to produce a given structure. There was no idea that the fate of these cells might be changed or that other cells might share it. The repeated attempts to discover an exact definition of the term archesporium will serve as an illustration; and the repeated failures should have warned sooner than they did. Indifference of primordia was not thought of, and each living cell was conceived of as having only a single possibility.

The idea of unvarying sequence and predestination not only entered into the conception of developing organs, but also directed an immense amount of work in connection with the early embryonic stages of both gametophyte and sporophyte. So far as my own experience is concerned, it was in this connection that the conception of rigidity broke down. The multiplication of observations caused definite sequence and predestination to vanish in a maze of variations. This type of morphology was necessarily its own corrective, for rigidity could not stand before the accumulation of facts. In a sense, rigidity of conception is easier to grasp and certainly simpler to present than flexibility of conception, for the human mind seems to demand its knowledge in labeled pigeon-holes. This same spirit permeated the attitude of the morphologist of this period towards his ultimate purpose, for phylogeny to him was rather a simple conception. Similarity of structure meant community of descent. Such a condition as heterospory, such a structure as the seed, or such an organization as the sporophyte was attained once for all, and the successful plant or group became the fortunate ancestor of all heterosporous plants, or spermatophytes, or sporophytes. This was phylogeny made easy. Multiplied observations showed that simi-

larity of structure often does not indicate community of descent, and we are staggered before the possibilities of phylogeny.

The division of morphology that we have been pleased to call cytology has had the same experience. It was hoped that the more fundamental structures would show some reasonable constancy of phenomena, some rigidity in detail; but we have been confronted here again by endless variation, and hence most diverse interpretation of results.

Clearly, belief in a rigid sequence or in predestination could not be maintained; and in a real sense morphologists have been cataloguing material for study, and their real problems lie behind these endlessly variable details.

The phase of morphology just described has certainly dominated during the last half century, with phylogeny as its chief stimulus, and a rigidity of conception that only a multitude of facts could break down. It is a type that must always exist, as taxonomy must always exist, and it must be considered fundamental in familiarizing with material; but, perhaps, it may be said now to be at its culmination as the dominant phase.

3. THE PHASE OF THE INFLUENCE OF CHANGING CONDITIONS UPON THE DEVELOPING ORGAN.

This means experimental morphology, and so far as organs are concerned its purpose is to discover the conditions that determine their structure and nature. All idea of rigidity has disappeared in the fundamental conception of the capacity of living cells to respond to varying conditions. What may be the possibilities of variation, and what may be the exact conditions responsible for variations, are questions to be answered by experiment. If the oldest morphology is in its decline, and the current morphology at its culmination, experimental morphology may be said to be in its inception. It is easier to judge of a movement at its decline or culmination than at its inception, and experimental morphology as yet is fuller of promise than of performance. In any event, it was an inevitable phase when multiplied variation had broken down the conception of rigidity. The fundamental question of the possibilities of living cells is immediately confronting us; and the range of these possibilities may be considered under three heads.

1. *The Varying Structure of an Organ.*—Perhaps leaf variation, which enters so largely into taxonomy, may be used as an illustration. When under experimentation leaves can be made to vary from narrow to orbicular, from dissected to entire, and the exact physical condition determined that induces the result, any idea of rigidity in the form or structure of an organ must disappear. An observed narrow range of variation in nature may be regarded as an indication of the narrow range of conditions rather than of the narrow range of possible response on the part of the organ. From this point of view an organ is represented by its essentials, without reference to its non-essentials, and so we are now thinking of sporangia in terms of sporogenous tissue, without reference to the presence or absence of a morphologically constant wall; of archegonia as axial rows of potential eggs, without concern for an exact morphological definition of the sterile jacket. The main question is, what determines the formation of sporogenous tissue rather than of sporangia; what determines the formation of eggs or sperms, rather than of archegonia and of antheridia?

2. *The Possibilities of Primordia.*—This has to do with what I have called the doctrine of predestination. It is more than a question as to the variable form or structure of an organ; it is a question as to

variable nature of an organ that may arise from a given primordium. When primordia that usually develop microsporangiate organs produce megasporangiate ones, or *vice versa;* when the same plant body produces sporangia or gametangia in response to conditions imposed by the experimenter; it becomes evident that primordia may be indifferent not only as to form, but also as to nature.

This meant a general unsettling of morphological conceptions. To find, for example, that a given cell is not set apart from its first appearance to function as an archesporial cell, but that there are as many potential archesporial cells as there are cells in an extensive tissue; and further to find that the archesporial cell when discovered by its functioning does not necessarily produce all the sporogenous tissue, is to abandon the idea of predestination and of defining structures on a rigid morphological basis.

3. *The Origin of Species.*—Probably the greatest triumph of experimental morphology thus far is that it has put the problem of the origin of species upon an experimental basis. The ability to vary, and to vary promptly and widely, when considered in connection with structures used by taxonomists, means new species under certain conditions. To analyze these conditions is a problem of enormous complexity, but to have the problem clearly before us is but the prelude to its solution. There is still a tendency to call things inherent that are not apparent, but this is a habit not easily outgrown, and such a problem as the origin of species will long have its convenient category of 'inherent tendencies.'

Certain conclusions are inevitable as one considers the perspective opened by experimental morphology.

In the first place, it would seem that what we have called 'biological laws' are also the laws of physics and chemistry, and the experimenter must be prepared to use all the refinements of method developed by physicists and chemists. Much of the work done in the name of experimental morphology is as yet crude in the extreme, and we are often left with a confusing plexus of conditions rather than with a satisfactory analysis. To grow plants, to observe certain results and to draw conclusions, too frequently means the arbitrary or ignorant choice of one factor out of a possible score to be found in the uncontrolled conditions.

In the second place, that phase of ecology which deals with what are called 'adaptations to environment' simply catalogues the materials of experimental morphology and must be merged with it. To retain it as a distinct field of work is to doom it to sterility, for it can only bear fruit as it becomes an experimental subject, and then it is experimental morphology.

In the third place, experimental morphology, with its background of physics and chemistry, is more closely related to physiology than it is to the older phases of morphology; which leads to the conclusion that the fundamental problems of morphology are physiological. We may look at the situation from either standpoint, and say that the most recent phase of morphology entrenches upon physiology, or that the boundaries of physiology must be extended enough to include morphology. To-day the two subjects are handicapped; for morphologists are not physiologists enough to know how to handle and interpret their material, and physiologists are not morphologists enough to know the extent and significance of their material. The training of the future must not differentiate these two subjects still further, but must combine them for effective results.

This modern tendency to cross old-established boundaries between subjects is evi-

dent everywhere. Physiology and chemistry have long possessed common territory; plant morphology and physiology have now found no barrier between them. This simply means that so long as we deal with the most external phenomena our subjects seem as distinct from one another as do the branches of a tree; but when we approach the fundamentals we find ourselves coming together, as the branches merge into the trunk. The history of botany, beginning with taxonomy, has been a history that began with the tips of the branches and has proceeded in converging lines towards the common trunk. The fundamental unity of the whole science, in fact, of biological science, however numerous the branches may be, is becoming more and more conspicuous. Already the old lines of classification have become confused, and one looking through any recent list of papers finds it impossible to classify them in terms of the old divisions. Investigators are now to be distinguished by particular groups of problems in connection with particular material, and all problems lead back to the same fundamental conceptions. In other words, the point of view is to be common to all investigators, and until it is common their results will not reach their largest significance.

A fourth consideration is the result of all this upon taxonomy. It seems clear to one who was originally trained in taxonomy, and who has passed through all the phases of morphology described above, that the conception of species has become so radically changed that a reconstructed taxonomy is inevitable. When the doctrine of types disappeared, and when experimental morphology showed the immense possibilities of fluctuation in taxonomic characters, the taxonomy of the past was swept from its moorings. Taxonomy must continue its work as a cataloguer of material, but to catalogue rigid concepts is very different from cataloguing fluctuating variations. The attempt to do the latter on the old basis is being attempted in certain quarters, but it soon passes the limit of usefulness and sets strongly towards the record of individuals. Some new basis must be devised, and it must be a natural and useful expression of the relationships of forms as suggested by experimental morphology.

That this history of the progress of morphology, just outlined, is a fair indication of general tendencies may be illustrated from plant anatomy. This subject, not well differentiated from plant morphology among the lower groups, has developed a very distinct field of its own among vascular plants. Its early phase was that of classification, in which types of tissues were rigidly defined. This definite catalogue of tissues continued to be used after evolutionary morphology was well under way, and morphologists gradually abandoned any serious consideration of it, just as they had cut loose from the old taxonomy. In text-books the juxtaposition of morphology upon an evolutionary basis and a little anatomy upon a strictly taxonomic and artificial basis became very familiar.

Recently a second phase of anatomy has begun to appear, and we find it upon an evolutionary basis. Investigation has passed from the study of mature tissues to the study of developing tissues, and the seedling is more important to the anatomist than the adult body. As in the corresponding phase of morphology, the fundamental conception of this new phase is the theory of recapitulation, and its ultimate purpose is phylogeny. It views tissues as morphology views organs, and is attacking the same general problems. In so doing it becomes a special field of morphology, no more to be separated from it than are morphologists who study the sporophyte to

be separated from those who study the gametophyte. It is simply the development of another line of attack upon morphological problems. This anatomical morphology, as it may be called, has yet to accumulate its share of results, and there is no region of morphology more in present need of investigators. From the small beginnings it has made it is evident that it must check the conclusions of the older morphology at every point. Even now no statement as to phylogeny can afford to neglect the testimony of anatomy.

This second phase of anatomy promises to be accompanied by a third, which finds its parallel and probably its suggestion in experimental morphology. In its incipient stage it is known as ecological anatomy, just as another phase of ecology preceded and then became merged in experimental morphology. Ecological anatomy can make no progress until it becomes an experimental subject, and then it is experimental anatomy, which holds the same relation to experimental morphology that evolutionary anatomy holds to evolutionary morphology. In other words, it is the same subject, with the same methods and purpose, and differing only in the structures investigated. And thus anatomy reaches the physiological basis, and as a part of morphology fills out the structures to be investigated from this standpoint.

There remains a region of ecology so vast and vague that it must be considered by itself for a time. It deals with such complex relationships as exist between soil, topography, climate, etc., on the one hand, and masses of vegetation, on the other. Just because it is vast and vague ought it to be attacked. The little incursions that have been made indicate the possibilities. It evidently includes some of the great ultimate problems. As yet it can not define itself, for it seems to have no boundaries. Its materials were evident but entirely meaningless in the earlier history of botany, for it needed all of our progress before it could begin to ask intelligent questions. By virtue of its late birth it promises to develop more rapidly than any other phase of botany. And yet, beyond the inevitable preliminary classification of material, its real progress is measured by its experimental work conducted upon a definite physiological basis. Tentative generalizations are numerous and necessary, but they are merely suggestions for experiment. When one understands the close analysis necessary in the simplest physiological experiment, the problems suggested by this phase of plant ecology are appalling; but I see in the whole subject nothing but the largest application of physiology to the plant kingdom.

And now that the various phases of botany all seem to rest upon physiology, it must be apparent that the most fundamental problems are physiological. It is only recently that the development of plant physiology has justified this relationship. Its own history has been one of progress from the superficial towards the fundamental, from the behavior of a plant organ to the behavior of protoplasm. And here it becomes identified with physics and chemistry; and in a very real sense botany has become the application of physics and chemistry to plants.

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*THE CONCEPTS AND METHODS OF SOCIOLOGY.**

To set forth in a brief paper the fundamental conceptions of any modern science is a difficult task. The difficulty increases as we pass from the relatively simple sciences that have to do with inorganic matter,

* An address delivered at the International Congress of Arts and Science, Department of Sociology, September, 1904.

to the highly complex sciences of life and of mind. And when we come to the phenomena presented by aggregations of living beings—phenomena of the interaction of mind with mind, phenomena of the concerted activity of many individuals working out together a common destiny—we have a subject for scientific study too many-sided, too intricate, for description in a few comprehensive phrases, and the scientific study itself arrives at fundamental conceptions only after a long and extensive process of elimination. Fundamental conceptions in such a field are necessarily general truths, expressing the relations that endless facts of detail bear to one another, or to underlying groupings, processes or causes. A brief account, therefore, of the fundamental conceptions of sociology, and of the methods available for the scientific study of society, must remorselessly exclude those concrete particulars that lend to our knowledge of collective life its preeminently real—its human—interest. It must be restricted to conceptions that are elemental, general and in a degree abstract.

Conforming to this necessity, I shall group the fundamental conceptions of sociology in three divisions, namely: (1) Concepts of the subject-matter of sociological study, that is to say, of society; (2) concepts pertaining to the analysis and classification of social facts, and incidentally to the corresponding subdivisions of sociological science; (3) concepts of the chief processes entering into social evolution, and of the inferred causes.

The word 'society' has three legitimate significations. The first is that of the Latin word *societas,* meaning 'companionship,' 'good-fellowship,' 'pleasurable consorting together,' or meaning the individuals collectively regarded that consort. Examples of society in this original sense are afforded by the commingling of familiar spirits at the tavern or the club, the casual association of chance acquaintances at the summer resort, the numberless more formal 'functions' of 'the season.' In the second signification of the word, 'society' is a group of individuals cooperating for the achievement of any object of common interest or utility, as, for example, a merchant guild, an industrial corporation, a church, a congress of arts and science. Finally, in the third signification of the word, 'society' is a group of individuals dwelling together and sharing many interests of life in common. A nest of ants, a savage horde, a confederation of barbarian tribes, a hamlet or village, a city-state, a national state, a federal empire—all these are societies within the third and comprehensive definition of the term. A scientific conception of society must lie within the boundaries fixed by these three familiar meanings, but it must seize upon and make explicit the essential fact, whatever it may be, that is a common element in all social relations.

At the present time we find in sociological literature two competing conceptions of the essential nature of society. They are known respectively as the organic and the psychological conception.

The organic conception assumes that the group of individuals dwelling and working together is the true, or typical, society, and that it is as much a unity, although made up of individuals, as is the animal or the vegetal body, composed of cells and differentiated into mutually dependent tissues and organs. Sketched in bold outlines by Herbert Spencer in his essay on 'The Social Organism' in 1860, the organic conception has been elaborated by Schäffle and Lilienfeld, and is to-day accepted as the working hypothesis of an able group of French sociologists, whose work appears in the proceedings of L'Institut international de Sociologie.

The psychological conception assumes that, whether or not the organic conception be true and of scientific importance, it fails to get to the bottom of things. It assumes that, even if society is an organism, there is necessarily some interaction of individual with individual, or some form of activity common to all individuals that serves to bind them together in helpful and pleasurable relations, and that this activity, instead of being merely physical, like the cohesion of material cells, is a mental phenomenon. It assumes that all social bonds may be resolved into some common activity or some interactivity of individual minds. It is, in short, a view of society as a mode of mental activity.

This is the psychological conception in general terms. It takes, however, four specific forms in attempting to answer the question: What definite mode of mental action is the most elementary form of the social relation?

According to the most pretentious of these answers, one that dates back to Epicurus, and lies at the basis of all the covenant or social contract theories of political philosophy, the psychological origin of society is found in a perception of the utility of association. It assumes that men consciously and purposely create social relations to escape the ills of a 'state of nature' and to reap the rewards of cooperation. This rationalistic theory offers a true explanation of highly artificial forms of social organization in a civil, especially an industrial, state, but it throws no light upon the nature of elemental, spontaneous cooperation. For this we must turn to the other three conceptions —all of them, I venture to think, modernized forms of certain very ancient notions.

According to one of these, the most elementary social fact is seen in the constraining power, the impression, the contagious influence that an aggregation, a mass, of living beings, exerts upon each individual mind. Society is thus viewed as a phenomenon closely allied to suggestion and hypnosis. This view of society is most fully set forth in the writings of Durkheim and Le Bon.

A third conception, identified with the life-work of our lamented colleague, Gabriel Tarde, assumes that impression, contagion, influence, as forms of the interaction of mind with mind, may themselves be accounted for. It explains them as modes of example and imitation. All society is thus resolved into products of imitation.

In strict psychological analysis these 'impression' and 'imitation' theories must be classed, I think, as scientifically developed forms of the 'sympathy' theories of society, that may be traced back through the literature of political philosophy to very early days. They offer proximate explanations of the great social facts of resemblance, of mutuality, of solidarity; but do they, beyond a doubt, trace concerted activity back to its absolute origin? Above all, do they account not only for similarity, but also for variation, for the differentiation of communities into leaders and followers, for competition as well as for combination, for liberty as well as for solidarity?

The fourth conception, put forth some years ago by the present writer, should be classed as a developed form of the instinct theory, dating back to Aristotle's aphorism that man is a political animal. It assumes that the most elementary form of social relationship is discovered in the very beginning of mental phenomena. In its simplest form mental activity is a response of sensitive matter to a stimulus. Any given stimulus may happen to be felt by more than one organism, at the same or at different times. Two or more organisms may respond to the same given stimulus simultaneously or at different times. They

may respond to the same given stimulus in like or in unlike ways; in the same or in different degrees; with like or with unlike promptitude; with equal or with unequal persistence. I have attempted to show that in like response to the same given stimulus we have the beginning, the absolute origin, of all concerted activity—the inception of every conceivable form of cooperation; while in unlike response, and in unequal response, we have the beginning of all those processes of individuation, of differentiation, of competition, which, in their endlessly varied relations to combination, to cooperation, bring about the infinite complexity of organized social life.

It is unnecessary to argue that this conception of society not only takes account of individuality as well as of mutuality, but that also it carries our interpretation of solidarity farther back than the theories of impression and of imitation, since both impression and imitation must be accounted for—in ultimate psychological analysis—as phenomena of reciprocal, or interstimulation and response. Indeed, the very language that Tarde uses throughout his exposition tacitly assumes as much. Example is stimulus, the imitative act is response to stimulus. The impression that the crowd makes upon an individual is stimulus, and the submission, obedience or conformity of the individual is response to stimulus. Moreover, the formation of the crowd itself has to be accounted for, and it will be found that, in many cases, the formation of a crowd is nothing more nor less than the simultaneous like-response of many individuals to some inciting event, circumstance or suggestion. In short, impression, imitation and conformity are specific modes, but not by any means the primary or simplest modes, of stimulation and response; and some of the most important phenomena of concerted action can be explained only as springing directly from primary like-responses, before either imitation or impression has entered into the process.

This conception meets one further scientific test. It offers a simple and consistent view of the relation between social life and the material universe. It assumes that the original causes of society lie in the material environment, which may be regarded as an infinitely differentiated group of stimuli of like-response, and, therefore, of collective action; while the products of past social life, constituting the historical tradition, become in their turn secondary stimuli, or secondary causes, in the social process.

A mere momentary like-response by any number of individuals is the beginning of social phenomena, but it does not constitute a society. Before society can exist there must be continuous exposure to like influences, and repeated reaction upon them. When this happens, the individuals thus persistently acting in like ways become themselves mentally and practically alike. But likeness is not identity. The degrees of resemblance or of difference in the manner of response to common stimuli manifest themselves as distinguishable types of mind and of character in the aggregate of individuals; while the differing degrees of promptitude and persistency in response have as their consequence a differentiation of the aggregate into leaders and followers, those that assume initiative and responsibility, and those that habitually look for guidance. These differences and resemblances have subjective consequences. Differing individuals become aware of their differences, resembling individuals become aware of their resemblances, and the consciousness of kind so engendered becomes thenceforth a potent factor in further social evolution.

Summarizing our analysis to this point, we may say that we conceive of society as

any plural number of sentient creatures more or less continuously subjected to common stimuli, to differing stimuli, and to inter-stimulation, and responding thereto in like behavior, concerted activity or cooperation, as well as in unlike, or competitive, activity; and becoming, therefore, with developing intelligence, coherent through a dominating consciousness of kind, while always sufficiently conscious of difference to insure a measure of individual liberty.

Which of these various conceptions of the ultimate nature of the social relation shall in the long run prevail must depend upon a certain fitness to account for all the phenomena of social life in the simplest terms. That fitness can be determined only through the further evolution of social theory.

But whatever the finally accepted view may be, there are certain classifications of social facts that may be accepted as among the elementary notions of any sociological system.

And first there are types or kinds of societies. The broadest groupings correspond to the familiar demarkations made by natural history. There are animal societies and human societies; and the human societies are further divided into the ethnic—or communities of kindred, and the civil—or communities composed of individuals that dwell and work together without regard to their blood-relationships.

More significant for the sociologist, however, is a classification based on psychological characteristics. The fundamental division now is into instinctive and rational societies. The bands, swarms, flocks and herds in which animals live and cooperate, are held together by instinct and not by rational comprehension of the utility of association. Their like-response to stimulus, their imitative acts, the frequent appearance among them of impression and submission, are all purely instinctive phenomena. Not so are the social relations of human beings. There is no human community in which instinctive like-response to stimulation is not complicated by some degree of rational comprehension of the utility of association.

The combinations, however, of instinct and reason are of many gradations; and the particular combination found in any given community determines its modes of like-response to stimulus and its consciousness of kind—establishes for it a dominant mode of the relation of mind to mind, or, as Tarde would have phrased it, of inter-mental activity. This dominant mode of inter-mental activity—inclusive of like-response and the consciousness of kind—is the chief social bond of the given community, and it affords the best distinguishing mark for a classification of any society on psychological grounds. So discriminated, the kinds of rational or human societies are eight, as follows:

1. There is a homogeneous community of blood-relatives, composed of individuals that from infancy have been exposed to a common environment and to like circumstances, and who, therefore, by heredity and experience are alike. Always conscious of themselves as kindred, their chief social bond is sympathy. The kind or type of society, therefore, that is represented by a group of kindred may be called the sympathetic.

2. There is a community made up of like spirits, gathered perhaps from widely distant points, and perhaps originally strangers, but drawn together by their common response to a belief or dogma, or to an opportunity for pleasure or improvement. Such is the religious colony, like the 'Mayflower' band, or the Latter-Day Saints; such is the partisan political colony, like the Missouri and the New Eng-

land settlements in Kansas; and such is the communistic brotherhood, like Icaria. Similarity of nature and agreement in ideas constitute the social bond, and the kind of society so created is therefore appropriately called the congenial.

3. There is a community of miscellaneous and sometimes lawless elements, drawn together by economic opportunity—the frontier settlement, the cattle range, the mining camp. The newcomer enters this community an uninvited but unhindered probationer, and remains in it on sufferance. A general approbation of qualities and conduct is practically the only social bond. This type of society, therefore, I venture to call the approbational.

The three types of society thus far named are simple, spontaneously formed groups. The first two are homogeneous, and are found usually in relatively isolated environments. The third is heterogeneous, and has a transitory existence where exceptional economic opportunities are discovered on the confines of established civilizations.

Societies of the remaining five types are in a measure artificial, in part created by reflection—by conscious planning. They are usually compound, products of conquest or of federation, and, with few if any exceptions, they are of heterogeneous composition. They are found in the relatively bountiful and differentiated environments.

4. A community of the fourth type consists of elements widely unequal in ability; the strong and the weak, the brave and the timorous, exploiters and the exploited—like enough conquerors and the conquered. The social bonds of this community are despotic power and a fear-inspired obedience. The social type is the despotic.

5. In any community of the fifth type arbitrary power has been established long enough to have identified itself with tradition and religion. Accepted as divinely right, it has become authority. Reverence for authority is the social bond, and the social type is, therefore, the authoritative.

6. Society of the sixth type arises in populations that, like the Italian cities at their worst estate, have suffered disintegration of a preexisting social order. Unscrupulous adventurers come forward and create relations of personal allegiance by means of bribery, patronage, and preferment. Intrigue and conspiracy are the social bonds. The social type is the conspirital.

7. Society of the seventh type is deliberately created by agreement. The utility of association has been perceived, and a compact of cooperation is entered into for the promotion of the general welfare. Such was the Achæan League. Such was the League of the Iroquois. Such was the confederation of American commonwealths in 1778. The social bond is a covenant or contract. The social type is the contractual.

8. Society of the eighth type exists where a population collectively responds to certain great ideals, that, by united efforts, it strives to realize. Comprehension of mind by mind, confidence, fidelity and an altruistic spirit of social service, are the social bonds. The social type is the idealistic.

Of these varieties of society the higher, compound communities, or commonwealths, may, and usually do, include examples of the lower types, among their component groups.

All of these eight types, and the instinctive type exhibited by animal bands, have been observed from the earliest times and have suggested to social philosophers as many different theories of the nature of society. Thus in the totemistic lore of savagery we find endless suggestions of an instinct theory. In the mythologies of tribally organized barbarians we find sym-

pathy, or natural brotherhood, theories, which later on are borrowed, adapted and generalized by the great humanitarian religions, like Buddhism and Christianity. Suggested by societies of congenial spirits we have the consciousness-of-kind theories, voiced in the proverb that 'birds of a feather flock together,' in the saying of Empedocles that 'like desires like,' in the word of Ecclesiasticus that 'all flesh consorteth according to kind, and a man will cleave to his like.' From approbational societies have come our natural-justice theories. From despotic societies have come our political-sovereignty theories that 'might makes right,' in the sense of creating law and order. From authoritative societies have come theories of the divine right of kings; from conspirital societies have come Machiavelian theories of the inevitableness of intrigue and conspiracy; and from societies long used to deliberative assemblies, to charters of liberty and bills of rights, have come the social-covenant or contract theories of Hobbes, Locke and Rousseau. Finally, from societies that have attained the heights of civilization have come the Utopian theories, from Plato until now.

Whatever the kind or type of the society, there are found in it four great classes or groupings of facts.

Every society presupposes a certain number of concrete living individuals. The basis of every society, therefore, is a population. Every social population offers for observation phenomena of aggregation, or distribution of density; phenomena of composition, by age, sex and race; and phenomena of amalgamation or unity.

The social life, however, as we have seen, is a phenomenon of mind, and the varied modes that the common activity and interplay of minds assume, present the second great class of social facts. These facts of the social mind, as we may call them, include the phenomena of stimulation and response in their generic forms; phenomena of resemblances and differences, that is to say, of types; phenomena of the consciousness of kind; and phenomena of concerted volition.

The common mental activity, taking habitual forms, creates permanent social relationships, that is to say, a more or less complex social organization. In this we meet the third great class of social facts. Two general forms may be observed. In one form, individuals dwell together in groups that, by coalescence and federation, compose the great compound societies. These groups collectively may be called the social composition. In the other form, individuals, with more or less disregard of residence, combine in associations to achieve specific ends. Such associations collectively represent the social division of labor, and, therefore, may be called the social constitution. In its entirety and in its subdivisions the social organization is of one or another type, according as it is on the whole coercive, or on the whole liberal, in character.

The fourth class of social facts pertains to the great end, to the attainment of which the social organization is a means. That end is the social welfare. The social welfare is seen in its most general form in certain public utilities, including security, justice and liberty, material prosperity and popular culture. It is seen finally in the type of personality that the social life creates, and which must be studied as vitality, mentality, morality and sociality.

Not every society individually considered survives long enough to pass through all the possible stages of social evolution, but society in the aggregate, and in historic continuity, displays to us four distinguishable stages of evolutionary advance. There is, first, the stage of zoogenic association,

in which the mutual aid and protection practised by animal bands plays an enormously important part in the differentiation of species and in the survival of those best endowed with intelligence and sympathy. There is, next, the stage of anthropogenic association, in which, through unnumbered ages, the creature that was destined to become man was acquiring the distinctly human attributes of language and reason. There is, later on, the stage of ethnogenic association, wherein is evolved that complex tribal organization characteristic of savage and barbarian life. Finally, there is the stage of civic or demogenic association, in which great peoples outgrow tribal organization, and create a political organization based on common interests, irrespective of blood-relationships.

These categories of social fact have established certain natural subdivisions in social science. Corresponding to the historical order we have, first, studies in animal sociology; second, studies of primitive human culture; third, the great sciences of ethnography and ethnology, investigating tribally organized mankind; and, fourth, history, the narrative and descriptive account of the evolution of civil society. Corresponding to the four great divisions of phenomena in contemporaneous society we have, first, demography, or the study of social populations; second, social psychology, and the culture-studies of comparative philology, comparative art, comparative religion, and the history of science, all of which are investigations of the social mind; third, the political sciences, devoted to a study of social organization; and fourth, such sciences of the social welfare as political economy and ethics, the scientific study of education, studies of pauperism and criminology.

Such being our conceptions of the nature of society, and of the proper analysis and classification of social facts, let us pass on to examine our concepts of the great processes of social evolution, and of the causes in operation.

We accept the evolutionist point of view, and regard all the transformations that occur within any social group as a phase of that ceaseless equilibration of energy taking place throughout the universe. Every finite aggregate of matter is in contact or communication with other finite aggregates, no two of which are equally charged with energy. From the aggregate more highly charged, energy is given off to aggregates that are undercharged, and in this process the strong absorbs, or disintegrates, or transforms, the weak. Every social group, animal or human, since time began, has been in ceaseless struggle with its material environment and with other social groups. Whatever has happened to it or within it is most intelligibly accounted for if we view the process as one of equilibration of energies, between the group and its environment, or between group and group, or between unequal and conflicting elements within the group itself.

The modes that this equilibration assumes are many.

There is, first, the external equilibration of the society with its surroundings. This gives rise to the processes of migration, in which populations move from place to place, in search of new food supplies. Social groups are thus brought into conflict with one another, and the activities of militarism are engendered.

There is, next, a process of combined external and internal equilibration. Migration is its chief manifestation, but the migration is not now one of entire populations organized for war and conquest. It is one of individuals or families, moving from land to land in search of economic opportunity or of religious or political liberty, and its consequence is that exceeding

heterogeneity of the demotic composition which is seen, for example, in the population of the United States.

There are, thirdly, the processes of internal equilibration. First among these is the differentiation of the mind of the population, consequent upon some degree of unlikeness and inequality in the responses of differing individuals to the common stimuli to which all are subjected. This is followed by the segregation of resembling products into types and classes. Secondly, there is an evolution of the consciousness of kind, with increasing attention to means of communication and association. Thirdly, there is a struggle between strong individuals and weak, between leaders and followers, between strong and weak classes. This equilibration may take one of three possible forms: (1) The subjugation and perhaps the enslavement of the weak by the strong; (2) economic exploitation; (3) the uplifting of the weak by the strong through education, justice and economic aid. The moral advance of society is a progress from equilibration through subjugation and exploitation to equilibration through uplifting, and it depends upon the broadening and deepening of the consciousness of kind.

A fourth phase of internal equilibration appears in the struggle among differing groups of the like-minded in the community. Some elements of the population are sympathetically emotional, or are alike in beliefs or dogmas. Others are alike intellectually, rationally; they attain agreement through deliberation. In every community the reasoning and the unreasoning elements are in perpetual conflict.

To the extent that the community is controlled by its deliberative element, it exhibits a policy—a more or less consistent attempt consciously made to control its destiny. In the history of human society there have been three great groups of policies, namely: (1) policies of unification—attempts to make all members of the community alike in type, in belief and in conduct; (2) policies of liberty—attempts to give wide scope to individual initiative; (3) policies of equality—attempts to prevent the disintegration of society through an excess of individual liberty. The struggle of conflicting interests in the community, which these three modes of policy represent, is yet another form of internal equilibration.

To the extent that a policy of equality is adopted, the community is democratic. Political equality, equality before the law, and some approach toward equality of economic opportunity, are the essential elements of democracy. No sooner is democracy evolved than we see a struggle between the forces that make for absolutist, and those that make for liberal, democracy. Either the majority is permitted to rule at will, or it is compelled to leave inviolate certain rights of the minority and of individuals.

The outcome of all equilibration, external and internal, is a certain relation of the individual to the social organization. In low types of society the individual literally belongs to the various social groups in which his lot is cast. He belongs to them for life. To leave them is to become an outcast. He may not leave his clan, his guild, his caste, his church, or his state. In superior types of society we discover a high degree of individual mobility combined with a marvelous power to concentrate enormous numbers of individuals in moments of emergency, upon any work needing to be done. The individual may go freely from state to state, from parish to parish, in search of his best economic opportunity. He may sever connection with his church to join another, or none at all. He may be a director to-day in a dozen corporations, and to-morrow in a

dozen different ones. The goal of social evolution is a complex, flexible, liberal organization, permitting the utmost liberty and mobility to the individual, without impairing the efficiency of organization as a whole.

On the methods of sociology remark at this time must necessarily be brief.

Dealing as we do with highly concrete materials, we place our main reliance upon systematic induction. The experimental method of induction, however, is of little avail in the scientific study of society. Although social experimenting is at all times going on, it is difficult to isolate causes or to control conditions with scientific thoroughness. Observation, therefore, and critically established records of observations made in bygone days, must be our main dependence, so far as the accumulation of data is concerned.

Yet in a field so vast, observation itself would be a fruitless toil if it were not directed by scientific rules. Canons of guidance we find in the so-called comparative and historical methods. Selecting any social fact, or correlation of facts, observed in any given society, we systematically search for a corresponding fact or correlation in all contemporaneous societies, animal and human, ethnic and civil. This search has one clearly defined object, namely, to determine whether the observed fact is a universal, and therefore an essential, an elementary phenomenon of society, and, if it is not universal, to ascertain just how wide its distribution is. By such research we discover those resemblances and differences in social phenomena that are the bases of scientific classification.

Having in this manner arrived at a scheme of classification, we use it in subsequent observation precisely as the chemist or the botanist uses the classifications that have been established in his science. We systematically look for the facts and the correlations that the classification leads us to anticipate.

In like manner, following the historical method, we search for a given social fact at each stage in the historical evolution of a given society, and thereby determine what social phenomena are continuous.

A complete scientific theory of natural causation is established only when our knowledge becomes quantitatively precise. Often the law that we seek to formulate eludes us until the correlations of phenomena have been determined with mathematical exactness. Sociology has unjustly been reproached for neglecting that attention to precision which is the boast of other sciences. The indictment of vagueness may be a true bill against individual sociologists. It is demonstrably not a true bill against sociology. It is to the scientific students of sociology that the world owes the discovery and development of an inestimably valuable form of the comparative and historical methods, namely, the statistical method. Every inductive science to-day is adopting this method. Physics, chemistry, astronomy and geology would be helpless without it. The biologists have acknowledged their dependence upon it by the establishment of a statistical journal, *Biometrica*. It is not too much to claim that the possibilities of this now indispensable method of all the sciences were first demonstrated in the epoch-making social studies of Jaques Quetelet, and that its employment in sociology has been out of all proportion to its employment elsewhere. As developed in recent years by Westergaard, the Dane; by Germans, like Steinhauser, Lexis and Meyer; by Italians, like Bodio; by Frenchmen, like Levasseur and Dumont; by Englishmen, like Charles Booth, E. B. Tylor, Galton, Bowley and Karl Pearson; by Americans, like Mayo-Smith, Weber, Norton, Cattell, Thorndike and Boas, it has become,

and will continue to be, the chiefly important method of sociology; and assuredly, in the course of time, it will bring our knowledge of society up to standards of thoroughness and precision comparable with the results attained by any natural science.

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*RECENT ADVANCES IN THE ANALYSIS OF THE EARTH'S PERMANENT MAGNETIC FIELD.**

The 'earth is a great magnet' and as such is subject to the same laws which pertain to any other magnet—these are facts established by the experience of over four centuries. How and whence the earth has received its magnetism are questions we can not as yet answer, nor, in my opinion, shall we be able to answer them definitely until we have solved the problems as to the causes of the *variations* of the earth's magnetism. I firmly believe that when we have discovered the causes of the periodic and aperiodic variations, such as the diurnal variation, annual variation, secular variation and magnetic perturbations, we shall have strong hints given us as to the *origin* of the earth's magnetism. It is through the study of the *variations,* then, that we hope some day to be able to attack the problem as to the origin with some degree of success. Until this study has been completed, it is not believed that anything more than mere surmises, such as the magnetic literature contains *in quanto* can be given.

Whether the earth is a magnet like a lodestone or an electromagnet, is another question which can not as yet be definitely answered, though there are various indications that the earth's magnetization partakes of the character of both. Here again the definitive answer depends upon the successful solution of the questions as to the variations of the earth's magnetism both as to time and space.

These introductory paragraphs are intended to emphasize the proposition that if progress is to be made in the subject of the earth's magnetism, we must first make a careful and exhaustive study of the facts which are daily experiences, before attempting broad, theoretical generalizations based on more or less inadequate data permitting at the most mere qualitative tests of the deductions of theory. What are needed are the facts for *quantitative* tests. Even then, it will be found, in some instances, that more than one theory will satisfactorily explain the same facts and that a final decision must be left to future generations. However, the *facts* will remain as a permanent acquisition. *The accumulation of clean-cut facts regarding the earth's magnetism is the great task of the present generation.*

In the hope of enlisting interest in this comparatively unexplored field of scientific inquiry, it will be my endeavor to reveal some of the gaps to be filled as well as to exhibit those facts considered as safely established. It must be remembered that we are working in a field bordering on several other sciences, such as astrophysics, geophysics, geology and meteorology, so that he who wishes to become an expert must have at his command the ability to make the best and most intelligent use of the experimental facts of several of the older, recognized sciences. The physicist now-a-days has no time to attempt to master so special and comprehensive a subject as that of the earth's magnetism, with its manifold ramifications into cognate sciences, for he finds it sufficiently difficult to keep in touch with the rapid advances in his own subject. However, if the physi-

* Presented before the joint session of Section A of the International Electrical Congress and the American Physical Society, at St. Louis, on September 23, 1904.

cist, the mathematician, the geologist or the astrophysicist has presented to him the problems of the earth's magnetism concerning him specially, definite advance along certain lines may be confidently expected. The point then made is that the successful solution of some of the vexing problems of the earth's magnetism, in this day of rapid advances in experimental research, can not be attempted by one individual; he must associate with him experts in several of the older, fundamental sciences and have at his command a staff of computers. It must, hence, be a source of great gratification that this dream has been realized in the establishment by the Carnegie Institution of a Department of International Research in Terrestrial Magnetism, with facilities for adequately and exhaustively collecting, collating, supplementing and discussing magnetic data. With such means, let us hope that before very long we may be able to present a more favorable report on the state of our knowledge regarding the earth's magnetism, than the one which can be given now.

One of the most fundamental inquiries to be made in the discussion of any of the earth's magnetic phenomena, before attempting a theoretical explanation, is as to the *seat* of the forces giving rise to the phenomenon in question. Thus many a theorist might have saved himself some pains had he first addressed himself to this inquiry. To illustrate:

Suppose our first question to be the following: Since we can produce the magnetic phenomena pertaining to the earth's so-called 'permanent' magnetism, observed on the surface, by a system of closed electric currents, where are these currents? Do they circulate around the earth below the surface or in the regions above us? We know, as a fact of common experience, that the end of the needle designated as the north-seeking end, or for short, the north end, points approximately towards the north. Hence, applying Ampere's rule, the electric currents necessary to produce this phenomenon must circulate around the earth from east to west, if they be *inside* the earth, and if they are, on the other hand, *outside* the earth, they must circulate from west to east. To determine where the currents really are we must resort to another well-known phenomenon, viz., that the end of the needle which points to the north dips below the horizon in our hemisphere and points above the horizon in the southern magnetic hemisphere. Applying to this phenomenon Ampere's rule, we shall find that the currents can only circulate from east to west, hence combining this deduction with our previous one, the answer is that the electric currents which are capable of producing the observed magnetic phenomena cited circulate from east to west inside the earth.

Now this is a perfectly simple and obvious application of a fundamental law in electromagnetism, and yet for want of this test many eminent investigators have lost valuable time and even to-day some cases of transgression or omission might be cited. Thus some of the theories of the secular variation suppose that the electric currents causing this variation are situated chiefly outside of the earth. However, according to recent calculations, as based virtually upon the mathematical application of Ampere's rule, it is found that the observed facts can be made to harmonize best with a system of forces situated chiefly inside the earth. [Since the reading of this paper, the calculations reveal the existence of also a minor system of outside currents taking part in the production of the observed secular variation.]

The first one to make a mathematical test of the seat of the earth's magnetic forces, coupled also with an analysis into

spherical harmonic terms to the fourth order, was Gauss, from whose time a new era in magnetic science was ushered in. As the result of his mathematical analysis, it was definitely proved that by far the greatest portion of the earth's permanent magnetism is to be referred to a system of forces inside the earth and, furthermore, that this system possesses a potential. There were thus deduced two great fundamental facts of nature that outweigh in importance all of the speculative theories concerning the 'how and whence' of the earth's magnetism.

Gauss's calculations have been repeated several times with the aid of more complete material by several analysts, one of them being the noted astronomer and mathematician, John Couch Adams; Gauss's deductions have been verified by all of them.

The most elaborate analysis and attempt at perfection of the theory embodied in the Gaussian analysis was that for 1885 by Professor Adolf Schmidt, at present in charge of the Potsdam Magnetic Observatory in succession to the late and lamented Professor Eschenhagen. Schmidt made provision in his equations: (*a*) For the effect of the spheroidal figure of the earth, Gauss having taken a spherical figure, (*b*) for a possible effect due to forces whose seat was outside the earth, and (*c*) for a possible effect not to be referred to an inside or outside potential, but to a system of vertical electric currents passing through the earth's surface, whether from inside or outside.

Schmidt found that about 95 per cent. of the total magnetization of the earth was to be referred to an inside potential and that the remainder was due to a small outside potential and an electric current system traversing the earth perpendicularly to its surface. [The writer has since found that the principal term of the outside potential is displaced about one hundred degrees (100°) to the *west* with reference to the principal term of the inside potential.]

Fritsche in the main verified Schmidt's work, though he did not introduce the refinement due to taking into account the spheroidal figure of the earth, but retained the simpler equations based on the spherical figure.

The writer has recently made a critical comparison of the results thus far obtained by the various analysts, and has derived the differences between the elements as computed upon the basis of the theory and the observed or chart quantities, his purpose being to ascertain wherein further improvement of the theory is needed and what direction promises the best success. The residuals exceed many times the errors of observation.

It would appear that at the present stage very little increased accuracy has been gained by taking into account the spheroidal figure of the earth and that the theory must receive elaboration in other fundamental directions. Thus, for example, suppose the principal portion of the earth's magnetic system to be situated at some considerable depth below the surface—a condition of which we in fact have indications—then the question must be considered as to the effect arising from the magnetic permeabilities of the strata intervening between the seat of the system and the place of measurement of the forces. Instead of having the simple Laplacian equation and, as the result of which, a strictly harmonic distribution of the forces, we may have instead the more generalized equation:

$$\frac{\partial}{\partial x}\left(\mu\frac{\partial V}{\partial x}\right)+\frac{\partial}{\partial y}\left(\mu\frac{\partial V}{\partial y}\right)+\frac{\partial}{\partial z}\left(\mu\frac{\partial V}{\partial z}\right)=0,$$

and in consequence a quasi-harmonic distribution. So that the Gaussian potential expression, based on the simple Laplacian equation, may represent only a first approximation to the truth.

The material for testing this hypothesis, however, is not yet either at hand or sufficiently complete.

And right here we must record a most lamentable condition of our knowledge regarding the general distribution of the earth's magnetic forces. One of the surprising results of my critical comparison, above referred to, was the fact that *the accuracy in the determination of the earth's magnetic potential is about the same whether we use the magnetic charts of Sabine of 1840–5 or the best modern magnetic charts.* In other words, whereas magnetic surveys have steadily progressed on land areas and even have been repeated in certain instances in greater detail, the magnetic survey of the great oceanic areas and of the unexplored land regions has made very little progress during the past half century. The advent of the iron ship has materially lessened the yield of useful magnetic data and the expeditions designed for securing sea results have been unfortunately too few and far between. In the Antarctic regions, for example, practically no progress had been made since the observations of Ross in the *Erebus* and *Terror* during the fourth decade of the last century until the recent Antarctic expeditions of the British and German empires.

Fortunately, however, there is an awakening interest in this direction. Thus a committee has been appointed by the International Association of Academies, at its recent meeting in London, to consider methods for securing increased accuracy in magnetic work at sea. Furthermore, the plans of the Department of Terrestrial Magnetism of the Carnegie Institution embrace cooperation in the magnetic survey of the oceanic areas, and it is confidently hoped that a beginning in this direction can soon be made. Instruments for this purpose have already been ordered. Also, I am glad to be able to announce that, having succeeded in organizing the detailed magnetic survey of the land area of our country, attention has next been paid to inaugurating similar work at sea on the Coast and Geodetic Survey vessels; three of them have already been fully equipped for this purpose and this fall two more will receive their magnetic equipments. While these vessels can only obtain magnetic data incidentally in the course of their surveying work, experience has shown that a very satisfactory degree of accuracy can be secured by their skilled officers. The magnetic declination and dip can be obtained, for example, to about 5′ to 10′ and the total force to about one five-hundredth part.

We next inquire, *is the earth's magnetic energy increasing or decreasing?* This is a question of fundamental importance to theories of the earth's magnetism. As is well known, the earth's magnetic elements are subject to a secular variation whereby considerable changes are produced in the course of time. A secular variation may result from a change in the intensity of the magnetization of the earth, or from a change of the direction of magnetization, or from both causes.

The first term of the Gaussian potential is of a simple harmonic type and constitutes by far the largest term; it represents about 65 to 70 per cent. of the total magnetization and can be physically interpreted as a uniform or homogeneous magnetization symmetrical about a diameter, inclined $11\frac{1}{2}°$ to the earth's axis of rotation. This diameter Gauss defined as the earth's magnetic axis, with respect to which he determined the magnetic moment due to the first term. Tabulating the values of the magnetic moment as derived for different epochs from the various analyses, we shall find that it has decreased in forty-six years by 1.6 per cent.—an alarming loss, if true!

The question now is, whether this apparent loss is in any way wholly or partially compensated for by a possible increase in magnetic energy of the portion of the earth's magnetism represented by the remaining terms of the Gaussian potential, *i. e.*, by the portion which can not be referred to a uniform magnetization about some diameter? If mutual compensation does not take place, what is the annual loss of the earth's total magnetization?

To answer this query, I have made use of the well-known function in physics giving the energy W, of a distribution of forces in terms of the field intensity, F, viz.:

$$W = \frac{1}{8\pi}\int \mu F^2 d\tau$$
$$= \frac{1}{8\pi}\iiint \mu(X^2 + Y^2 + Z^2)\,dx\,dy\,dz,$$

where μ is the magnetic permeability and $d\tau$ is the element of volume and X, Y, Z are the rectangular components of F. The integral is confined to the space outside of earth, so that we may take μ as a constant and set it equal to 1. We may also give the expression the following form:

$$W = -\frac{R^2}{8\pi}\int VZ ds.$$

Here V is the magnetic potential and Z the vertical force on the earth's surface, R the earth's mean radius and ds the element of surface. As a check I have made some of the calculations with both forms and have gotten, of course, identical results.

The following table gives the values of the magnetic energy as derived for the various epochs and as dependent upon the best of the analyses thus far made:

Table I. Values of the Earth's Total Magnetic Energy in C. G. S. Units (Ergs).
(The tabular numbers are to be multiplied by the cube of the Earth's mean radius.)

No.	Computer of Potential.	Epoch.	I.	II.	III.	IV.	II + III + IV.	Total.
1	Erman-Peterson......	1829	.03562	.00026	.00015	.00004	.00045	.03607
3 and 6	Adams.....................	1842.5	3590	29	15	3	47	3637
4	Fritsche....................	1842.5	3614	28	14	3	45	3659
8	Adams	1880	3481	36	17	4	57	3538
9	Fritsche..................	1885	3472	34	15	4	53	3525
5 and 11	Neumayer-Petersen..	1885	3464	35	16	4	55	3519
10 and 13	Schmidt......	1885	3494	34	15	4	53	3548
	Mean of first three....	1838	.03589				.00046	.03635
	Mean of last four....	1884	3478				54	3532
	Change in...............	46 years	—.00111				+.00008	—.00103

Hence we have for

	Earth's Total Magnetic Energy.
1838	0.03635 R^3 Ergs
1884	0.03532 R^3 Ergs

or a loss of 0.00103 R^3, or 2.88 per cent., or about one thirty-fifth part in forty-six years. This result is a startling one, for, if true and if the loss in the earth's magnetization continued at the same rate as prevailed during the period 1842–1885, it would imply that the earth will have lost its magnetic energy in about 1,600 years; hence extreme caution should be employed before reaching a definite conclusion. I have made some attempt to ascertain whether this loss can be accounted for by the difference in the material used in the construction of the various charts, and while it would appear that the loss is greater than the effect due to the difference of material, I am unwilling at present to announce a definite conclusion, but think it best to leave this question, at present, open.

Allusion was made above to the *possible*

existence of vertical electric currents passing through the earth's crust, as revealed by Schmidt's analysis. He found that there was on the average for the entire earth, for every square kilometer of surface, a current of one sixth of ampere, passing perpendicularly through the surface, either from the air into the earth or *vice versa.* However, as certain investigators found it difficult to harmonize a current of this strength with the known phenomena of atmospheric electricity, and since similar investigations conducted over well-surveyed, though restricted, areas by several eminent magnetists did not reveal these currents, Schmidt was led to doubt his result and ascribe it to systematic map errors.

The existence of these currents is revealed by the non-vanishing of the line integral of the magnetic force taken around a closed curve on the earth's surface. Such line integrals serve as a test of the hypothesis of a potential, as was first shown and approximately applied by Gauss. Let us choose, as our circuit, a parallel of latitude, and let us call, as is customary, the component of the horizontal magnetic force resolved in a west-east direction, the Y component, then, if $d\lambda$ is the element of the parallel,

$$\int_0^{2\pi} Y d\lambda = 0,$$

if the earth's entire magnetic force is due to a potential. If, on the other hand, electric currents of the kind mentioned exist, then, if I represents the total amount of electricity passing per second of time through the zone from the north geographical pole down to the parallel around which the circuit is made, expressed in electromagnetic units, we have:

$$I = \frac{1}{4\pi}\int_0^{2\pi} Y d\lambda.$$

In a paper published in 1897 I computed the values of I for every fifth parallel from 60° N. to 60° S. as based on Neumayer's magnetic charts for 1885, and also gave a graphical representation along a meridian of the average distribution of the currents found. The resulting system was such a methodical one as to strongly suggest that there might be some truth, after all, in the existence of vertical earth-air electric currents.

With the aid of the facilities of the Department of Terrestrial Magnetism of the Carnegie Institution, I recently have had my calculations for 1885 repeated for two other epochs, viz., first, as based upon Sabine's magnetic charts for 1840–5, which depended upon magnetic data distributed over about seven decades, with the date 1840–5 about in the middle of the series and secondly, as based upon Creak's charts for 1880 issued just after the magnetic results of the *Challenger* expedition were available to him.

A further check upon the computations was obtained by a consideration of the magnetic declination charts alone, viz., for four epochs—Sabine (1840–45), British Admiralty (1858), Creak (1880) and Neumayer (1885). The calculations were based on the following principle: the downward electric currents will deflect the north end of a magnetic needle to the west, whereas the upward currents will deflect the north end to the east. The results obtained thus, agreed well with that obtained from the Y components.

The mean results as derived from all the computations are given in the table on the following page.

For example, through the region of the earth between the parallels 50° north and the equator, the resultant quantity of electricity passing every second of time from the air into the earth amounts to 419×10^4 amperes. In the zone between the two parallels 50° N. and 40° N., the resultant currents are upward and the total amount

TABLE II. VERTICAL EARTH-AIR ELECTRIC CURRENTS.

[Plus sign means upward currents; whereas minus sign implies downward currents.]

Zone.	I in 10^4 A.	i in A per sq. km.
50 N. to Equator	—419	
40 N. to Equator	—539	
30 N. to Equator	—544	
20 N. to Equator	—313	
10 N. to Equator	— 90	
Equator to 10 S.	+105	
Equator to 20 S.	+203	
Equator to 30 S.	— 9	
Equator to 40 S.	— 86	
50 N. to 40 N.	+120	+.038
40 N. to 30 N.	+ 5	+.001
30 N. to 20 N.	—231	—.057
20 N. to 10 N.	—223	—.052
10 N. to Equator	— 90	—.020
Equator to 10 S.	+105	+.024
10 S. to 20 S.	+ 98	+.023
20 S. to 30 S.	—212	—.053
30 S. to 40 S.	— 77	—.021

of electricity passing per second of time from the earth to the air is 120×10^4 amperes; dividing the latter quantity by the total area of the zone, the upward current is found to average for the zone 40 N. to 50 N., 0.038 ampere per square kilometer. The quantities i in the last column give a maximum downward current in the zones 20 N. to 30 N. and 20 S. to 30 S., and upward currents near the equatorial belts, and again beyond parallels 30°.

The general conclusion to be drawn appears to be:

All of the modern magnetic charts—i. e., since those of Sabine for 1840–5—unite in indicating the probable existence of vertical earth-air electric currents of the average intensity over the region 45° N. to 45° S. of one thirtieth of an ampere per square kilometer of surface. These currents of positive electricity proceed upward (from the earth into the air) near the equatorial regions where there are ascending air currents, and downward near the parallels 25° to 30°, i. e., in the regions of descending air currents. Near the parallels 40° the electric currents are again upward, thus corresponding once more with the general atmospheric circulation. Beyond the parallels 45° the results appear too uncertain to warrant drawing a definite conclusion.

If it be true that the vertical electric currents are to be associated with air currents, and are hence convection currents, the importance of choosing circuits for testing the validity of the potential hypothesis in localities of steady air currents is made manifest. It is thus clear that meteorological conditions may play an important part—as already pointed out in my 1897 paper—in investigations as to the existence of vertical electric currents from magnetic surveys over limited areas.

In order to make some tests as to the manner of distribution of the upward and downward electric currents, the currents over quadrilaterals bounded by two parallels 10° apart and two meridians, likewise 10° apart, have been derived for the entire region from 60° N. to 60° S., for the three epochs 1842, 1880 and 1885. As a general result, it did not appear as though the directions of the electric currents—whether up or down—were to be associated with the distribution of land and water. There was, however, a decided indication, *for each epoch*, that over the areas of low pressure, where the air-currents are upward, there the electric currents were likewise, in general, upward, and that over the areas of high pressure where there are descending air-currents, there the electric currents were likewise descending.

Thus, as the average result, from the three epochs we have:

Region.		Quantity of Electricity
60° N. to 60° S.	for areas of low pressure:	$+829 \times 10^4$ amperes
	" " " high " :	-638×10^4 "

(+ means upward electric currents; —, downward electric currents.)

The average effect of electric currents for the region 45° N. to 45° S. is on the

east-west component of the earth's magnetic force (Y), 0.001 C.G.S. unit, or about one fiftieth of the average value of Y. The average effect on the horizontal intensity is about one one-thousandth part, *i. e.*, on the order of the error of a field determination. However, the average effect on the declination is about 0.2°—about six times the error of a reduced field determination of the declination on land, and about one to two times the error of a determination at sea by the most approved methods.

Having given the results to be deduced from a mathematical analysis of the earth's permanent magnetic field in accordance with the principles laid down by Gauss, let us now briefly turn our attention to another *mode of attack with the purpose of deriving physical interpretations of the various harmonic terms entering into the Gaussian expression.* The general title of the series of the papers devoted to this subject, of which the fourth number appeared in the September issue of the journal *Terrestrial Magnetism and Atmospheric Electricity*, is 'The Physical Decomposition of the Earth's Permanent Magnetic Field.'

The first harmonic finds a ready physical interpretation: it represents that entire portion of the earth's total magnetization which can be referred to a uniform homogeneous magnetization of the earth about a diameter inclined to the axis of rotation. This term represents about 65–70 per cent. of the total field. Let us term it the primary or 'normal' field.

The diameter or axis of magnetization of this field for 1885 made an angle of 11° 25.7′ with the rotation axis and pierced the northern hemisphere in longitude 68° 30.6′ W. of Greenwich. Its magnetic moment was 0.32298 R^3, C.G.S. units, R being the earth's mean radius. These figures were dependent on Schmidt's analysis of the earth's permanent magnetism, and a slight revision would be required in accordance with his latest published Gaussian coefficients. However, as it was found that these slight revisions are on the order of error of determination, it will, therefore, not be worth while at present to make any change.

In No. II. of the series of papers alluded to, it was shown how the determinations of the magnetic axis and of the magnetic moment were dependent upon the portion of the earth considered in the calculations, so that strictly the quantities adopted apply only to the area embraced. Fortunately, however, the effect of the neglected portions of the earth—the polar regions—diminishes rapidly with advancing latitude, so that the values as adopted for the primary field, depending as they did upon data from 60° N. to 60° S., will not differ sufficiently from those obtained, had there been data over the entire globe, to vitiate the general deductions regarding the characteristics of the 'residual' or 'secondary field,' *i. e.*, that portion of the earth's total magnetization remaining after deducting the homogeneous magnetization (the first term).

The map of this residual field has now been constructed for three epochs; first, for 1885 and recently also for 1842 and 1880, the first depending on Neumayer's magnetic charts for 1885, the second on Sabine's charts and the third upon Creak's charts. The maps of the residual field for the first two epochs agree well in all the principal features with the one for 1885.

The residual magnetization can thus be broadly characterized: it consists chiefly of two main magnetizations transverse to the axis of rotation, one system lying in the northern hemisphere, the north end attracting pole (N_1) being east of the south end attracting poles (S_1', S_1''), and the other in the southern hemisphere, the direction of magnetization being the reverse

of the former, the north pole (N_2) lying now west of the south pole (S_2). The poles of the two systems are situated, approximately, near the 40° parallels—this is even true of the tertiary system N_3S_3.

The secondary magnetic equators (the lines along which the residual vertical force is zero) occupy practically the same positions for the three epochs. It is as yet too early to decide as to any probable secular shifting of the positions of the secondary poles. The interval is too short, in view of the meagerness of the data on which the charts depend, to make certain any deductions.

What has thus far been gained by the decomposition of the earth's total magnetic field into a primary and into a secondary one?

In the first place, the residual field clearly exhibits the fact that it is not a heterogeneous one, but, in general, remarkably systematic in its structure. There is, therefore, a very strong indication that it is produced by some distinct physical cause operating in the same general manner over the entire earth. The hope is thus clearly held out that we may still further resolve the residual field, starting with fundamental, physical causes.

My present belief is that the chief physical cause of the residual field is to be referred to the distribution of temperature within the stratum of the earth's crust here concerned.

There is a very remarkable correspondence between the principal features of the residual magnetic field and those exhibited on a chart of isabnormal temperatures. It is found that the earth as a magnet acts like any other magnet as regards application of heat. Thus, wherever the earth's surface is relatively warm, on the average for the year, there the magnetization of the earth shows a decrease, and where, on the other hand, it is relatively cold, there it suffers an increase. The comparison held so far, that it is possible to reproduce the residual magnetic field, in its general characteristics, with the aid of temperature charts.

The criticism has been made that this relation between residual magnetism and temperature distribution may only be an apparent one, since the latter referred to surface conditions, whereas the former pertained to strata at considerable depths below the surface. However, the isabnormal temperatures plotted were based on annual means; hence the effects due to annual variation and diurnal variation were eliminated. I am not aware that any one has given a physical explanation of the situations of the maxima and minima shown on an annual isanomalous temperature chart. Their annual positions are probably largely dependent on the radiation of the internal heat of the earth. We can not say, as yet, at what depth the principal thermal features shown at the surface are eliminated; it is known that the isothermal surfaces in the interior conform with those of the surface to a considerable depth. In any case, there is no question that as land areas are pierced, a steady increase of temperature is encountered. Over oceanic areas, on the other hand, there is at first a decrease until nearly a zero temperature is reached at the ocean beds, and then, presumably, an increase as the penetration continues. So that we shall have temperature gradients along parallels of latitude down to a considerable depth.

I shall not discuss this matter further now, as it is being made the subject of a special examination. Many have surmised that the distortion of the earth's magnetic field is to be attributed to the distribution of land and water; but the problem is to show in what manner the distribution causes the observed effects. The first attempt, as stated, will be to ascertain whether

the cause is to be sought in the distribution of temperature in the upper stratum of the earth's crust, as produced largely by the distribution of land and water. *The results of the decomposition have thus revealed one promising mode of attack of the problem as to the causes of the asymmetrical distribution of the earth's magnetism.*

Another extremely interesting result is that a very close similarity is found to exist between the chart of the residual permanent magnetic field and that of the system of forces causing the diurnal variation of the earth's magnetism. The two magnetic systems are identical in their general characteristics except in one respect, viz., the first is to be referred to a system of magnetic forces in the earth's interior, whereas the second to a system outside, the relative positions of the poles being governed accordingly. Thus at Greenwich mean noon, for example, the north end attracting pole of the first system would be about vertically below the south end attracting pole of the second system, and the south end attracting pole of the first would be about directly below the north end attracting pole of the second system—this statement holds for the main transverse magnetization in each hemisphere.

There appears to be more than a chance connection in this relation, as is shown by the horizontal vector diagrams for various parallels as resulting from the two respective fields.

I have had the impression for some time that the earth's permanent magnetic field may play a very important part in the production of the diurnal variation field as observed on the earth's surface. No satisfactory explanation has as yet been given of the manner in which the peculiar magnetic system of forces causing the diurnal variation is actually produced. Schuster's first attempt at the construction of the equipotential lines of the diurnal variation field, based as it was on exceedingly meager data, was, nevertheless, remarkably correct in its general features, as shown by the recent more elaborate work of Fritsche. We, therefore, have now a fairly accurate map of this field.

The existence of some form of radiation from the sun which does not penetrate to the lowest strata of our atmosphere, and which is yet capable of deflecting magnetic needles on the earth's surface, appears to have been definitely proved by the recent magnetic observations during solar eclipses. It was, furthermore, shown that the eclipse magnetic variation was a phenomenon similar to the diurnal variation, and that it differed from the latter only in degree; the ranges in the declination variations, for example, were proportional to the amounts of radiation cut off by the respective bodies: the moon and the earth.

It is known how a moving electrified particle will be deflected by a magnetic field, and how, in general, it will be made to travel in a spiral path whose axis is the line of magnetic force. Is it possible now, that as a result of the combined action of the permanent magnetic field of the rotating earth and the electrified particles radiated by the sun, there is formed in the regions above us a secondary magnetic system precisely similar to that of the earth?

The physical analysis of the permanent field, in addition to furnishing a number of interesting results, thus leads us, in a seductive manner, to the consideration of forces and phenomena not hitherto associated with those of the permanent magnetic field. We are led to inquire as to the rôle played, in the economy of nature, by the magnetic energy stored up in the regions outside, due to the earth's permanent magnetic field, in preventing certain solar radiations from reaching the lower

strata of our atmosphere. And at this threshold it will be well for us to pause and defer further exploration to a future time.

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SCIENTIFIC BOOKS.

Educational Psychology. By Edward L. Thorndike, adjunct professor of genetic psychology in Teachers College, Columbia University. New York, Lemcke and Buechner. 1903. 173 pp.

This book 'attempts to apply to a number of educational problems the methods of exact science' and to place at the service of students the incoherent mass of psychological knowledge which constitutes 'the beginnings of what we may call a general dynamic psychology.' That the facts already known "have thus far gone without systematic and convenient exposition is due to the complexity of the problems involved, not to any doubt concerning their practical importance. What we think and what we do about education is certainly influenced by our opinions about such matters as individual differences in children, inborn traits, heredity, sex differences, the specialization of mental abilities, their interrelations, the relation between them and physical endowments, normal mental growth, its periodicities, and the method of action and relative importance of various environmental influences." These then are the topics with which the book is concerned.

The first chapters deal with the measurement of mental traits. They explain the statistical principles by which we can apply exact measurements to groups of variable objects taken as a whole, and then proceed to show that in the matter of mental traits, as of physical, human beings of the same sex and approximately the same race, age and experience constitute a true group—that for every mental trait there is a mean or 'center of gravity' from which slight variations are frequent and great variations rare, according to the law of the distribution of chance events; so that measurements in terms of percentile grades or other relative standing can be interpreted fairly accurately in terms of absolute amount, and causal agencies acting unequally on different parts of the group can be detected by the analysis of curves of distribution.

In the fourth chapter the author explains the statistical principles by which we can measure the correlation between different characteristics in a group of individuals, and then applies Pearson's coefficient to show that in the matter of mental traits this correlation is amazingly small. This means that within a given group of individuals goodness or badness in one psychological function (as shown by school grades or the results of special tests) is not particularly likely to be accompanied by a similar amount of goodness or badness in another, even though the two appear to be only slightly different. If any one is disposed to deny this, let him first go over the large amount of evidence (much of it based on his own incessant investigations) which Dr. Thorndike puts together in this chapter.

Ever since the appearance of Professor James's chapter on memory the doctrine of general mental functions which can be cultivated by appropriate exercise and then turned to any use (a survival of the old 'faculty' psychology) has been more and more discredited amongst psychologists, though college presidents still make use of it to proclaim their wares and teachers in the public schools have never dreamed of anything else. But this doctrine can not be reconciled with the small coefficients of correlation shown in chapter IV. If there is such a thing as training or neglecting 'the memory' we should expect to find goodness or badness in remembering words accompanied by something like the same amount of goodness or badness in remembering numbers, to say nothing of colors, forms and tastes. But as a matter of fact the correlation is 'slight and variable'; and so with other functions.

In chapter VIII., on 'The Influence of Special Forms of Training upon More General Abilities,' the author presents evidence bearing still more directly upon this important educational doctrine, and gives a powerful argument to those who would adapt school

programs to the needs of life as directly as possible. If successful practise in guessing the size of paper squares less than four inches long gives relatively little improvement in the ability to guess the size of squares a trifle larger; if practise in marking all the words on a page which contain two given letters is very effective so far as these two letters are concerned, but gives only one third as much increase of speed for marking other letters, and still less increase of accuracy (in some cases accuracy in one function was actually diminished by the practise which increased it for a similar function); and if such improvement as there is can be accounted for by the overlapping of absolutely identical elements in the very closely allied activities tested; if, in short, there is practically no such thing as accuracy or thoroughness or concentration in general any more than there is memory or reasoning power in general, but only an unlimited number of relatively independent mental reactions, then the college president must cease promising, much like Protagoras of old, to give the young man or woman who comes to him 'a better disciplined mind for whatever work in life he may turn his attention to,' and the schoolmaster who wishes to give his pupils a 'well-rounded' education must learn to define the rotundity in terms of something other than a 'symmetrical development of faculties,' and not attach too much importance to so-called 'disciplinary' studies. *A propos* of these, it is interesting to notice that efficiency in mathematics bears no more relation to a schoolboy's general standing than does efficiency in any one of several other studies, and not so much as efficiency in English. Indeed, tests show that there is not much correlation between efficiency in two different mathematical studies, or even between two different processes in arithmetic; so great is the specialization of mental functions!

The chapter on the Inheritance of Mental Traits clears away theoretical objections to the possibility of such a thing and then shows by statistics, taken largely, though not altogether, from Galton and Pearson) that the influence of heredity is tremendous, though such evidence as is available points decidedly against the inheritance of acquired characteristics. "Original nature springs from original nature. Its improvement depends on the elimination of the worse, not on their reformation"; and yet "A college president can get hundreds of thousands of dollars to teach men various accomplishments, but he would be laughed at if he asked for $10,000 to prevent the most gifted young man in the college from remaining childless until 35."

The chapters on the relations of mental traits to age, sex and physical peculiarities are hard to summarize. They contain considerable positive material, but, like the chapter on Broader Studies of Human Nature, their main function is critical. They direct attention to unsolved problems, suggest methods of inquiry, and point out the errors by which various investigations have been vitiated; thus reminding us that, after all, we have only 'the beginnings' of a dynamic psychology.

Some such work as this of Thorndike's has been sadly needed. If the methods that it explains are all familiar to statisticians they are certainly unknown to many psychological investigators who deal with statistics, and if the criticism to which it subjects much work already done is pitiless, it is honest and fearless, and the constant expositions of method leave no excuse for slipshod work in the future.

As a text-book for students this 'Educational Psychology' can be used with great success. To be sure, it does not raise the reader to the heavens in a cloud of soul-stirring generalities, and there are some students who find many statistics somewhat overwhelming and frequent negative conclusions discouraging. But for students as well as for teachers of education it is worth while to know exactly where we stand and how great our ignorance often is; and those who have read the book and taken the trouble to master it appreciate its scientific spirit and its fiber.

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SOME RECENT LITERATURE ON THE STONY CORALS AND A REVIEW OF 'STEINKORALLEN' VON DR. EMIL VON MARENZELLER.*

During the time that has elapsed since the beginning of 1902, students of the *Madreporaria* have shown an unusual activity, and quite a number of very valuable papers, approaching the group from various sides, have been published. A few of the most important will be mentioned. Dealing with fossil corals there are, Lebedew's 'Die Bedeutung der Korallen in den devonischen Ablagerungen Russlands,'† Felix's splendid work on 'The Cretaceous Corals of the Gosau,'‡ and many shorter papers. During this period Duerden has published the results of his elaborate investigations on the 'West Indian Madreporarian Polyps."§ Verrill is the author of several interesting memoirs on West Indian, Bermudan and Brazilian corals,|| and 'Notes on Corals of the Genus *Acropora*.'¶ Gardiner has published 'South African Corals of the Genus *Flabellum*'** and Parts I. and II. of his '*Madreporaria*' of the Maldive and Laccadive Archipelagoes.†† Döderlein has published an excellent memoir on the genus '*Fungia*.'‡‡ Bernard has given us a most painstaking and elaborate monograph on the genus *Goniopora*.§§ We are indebted to Alcock for an excellent paper on the 'Deep-Sea *Madreporaria* of the Siboga Expedition.'* Besides these longer contributions on recent corals, a large number of shorter ones have appeared.

Never before have the *Madreporaria* received so much earnest attention. The group is being studied in almost every conceivable way by able investigators. The standard of excellence that prevails in these modern contributions is very high, it probably has not been excelled. But before the ultimate goal is attained, *i. e.*, the understanding of the relation of the organisms to their environment, the unraveling of the geologic history of the group and the formulation of a classification along philogenetic lines—much more work must be done. However, the outlook was at no previous time so bright as it is now. The one side that most demands attention is that of experimental physiological work—experimental study of the effects of environment on variation. These problems could be taken up by some of the marine laboratories. Probably much could be done by the Carnegie Laboratory on the Dry Tortugas.

Another valuable paper, Dr. von Marenzeller's 'Steinkorallen' of the *Valdivia* expedition, has just come into my hands.

The *Valdivia* obtained with 33 hauls of the dredge 29 species of stony corals. The depth varied from 44 to 2,278 meters; in only eight cases was it greater than 900 meters; in two cases it was less than 100.

There were six stations in the Atlantic Ocean, distributed from the west coast of Scotland to the Cape of Good Hope. The other stations are divided into five groups:

I. Cape Angulhas and Angulhas Bank, southern Africa.
II. Bouvet Island, Lat. 54° 29′ 3″ S., Long. 6° 14′ E.
III. Islands of St. Paul and New Amsterdam, in the southern Indian Ocean.
IV. West of Sumatra.
V. From Dar-es-Salam northward along the eastern coast of Africa.

It will thus be seen that the collections were made in the Atlantic and Indian oceans.

* *Wissenschaftliche Ergebnisse der deutschen Tiefsee-Expedition* auf dem Dampfer *Valdivia*, 1898–1899, Bd. VII., pp. 261–318, Taf. xiv–xviii, 1904.

† 'Mem. Comité Geology.,' St. Petersburg, Vol. XVII., No. 2, pp. ix, 1–180, 5 pls., 1902.

‡ 'Die Anthozoen der Gosauschichten in den Ostalpen,' *Palæontographica*, Bd. xlix, pp. 163–360, 19 Taf., 1903.

§ 'Mem. Nat. Acad. Sci.,' Washington, Vol. VIII., pp. 399–597, 25 plates, 18 figs., 1902.

|| *Trans. Conn. Acad. Sci.*, Vol. XI., pp. 63–206, 26 pls., 1902.

¶ *Ibid.*, pp. 207–266, 7 pls., 1902.

** 'Marine Investigations in S. Africa,' Vol. II., No. 6, pp. 113–154, 4 pls., 1902.

†† Reviewed in this Journal, Oct. 14, 1904.

‡‡ 'Die Korallengattung Fungia,' *Senckenberg. Naturfors. Gesellsch.*, Bd., XXVII., Heft I., pp. i–iv, 1–162, 25 Taf., 1902.

§§ 'Brit. Mus. (Nat. Hist.) Cat. Madreporarian Corals,' Vol. IV., pp. i–viii, 1–206, 14 pls., 1903.

* 'Siboga-Expeditie,' Mon. xvia, pp. 1–51, 5 pls., 1902.

The Bouvet Island locality is considered especially interesting because it lies so far south and connects with the Antarctic Madreporarian fauna. One species, *Caryophyllia antarctica* n. sp., was found there.

After the remarks concerning the areas from which the corals were obtained, are: 'List of the stations at which *Madreporaria* were collected,' for each station the latitude and longitude, depth, an occasional remark on the character of the bottom, and the species found there are given; 'The depth at which the *Madreporaria* occurred,' the species are listed according to the depth at which they were found; 'List of the species,' each species named being followed by the appropriate station numbers.

Then a detailed consideration of the species is given. The following is a list of the genera, with the number of species referred to each and the names of those described as new:

Desmophyllum, 1; *Flabellum,* 7, *F. deludens* nov., *F. stabile* nov., *F. chunii* nov., *F. magnificum* nov., *F. inconstans* nov.; *Sphenotrochus,* 1, *S. aurantiacus* nov.; *Deltocyathus,* 1; *Caryophyllia,* 5, *C. antarctica* nov.; *Stenocyathus,* 1; *Aulocyathus* gen. nov., 1, *A. juvenescens,* nov.; *Ceratotrochus,* 1, *C. delicatus* nov.; *Stephanotrochus,* 2, *S. campaniformis* nov., *S. explanans* nov.; *Lophohelia,* 1; *Amphihelia,* 1; *Solenosmilia,* 1; *Parasmilia,* 1; *Bathyactis,* 1; *Balanophyllia,* 1; *Dendrophyllia,* 1; *Coenopsammia,* 1; *Anisopsammia* gen. nov., 1.

The new genus *Aulocyathus* resembles *Schizocyathus* Pourtalès in external appearance; each of the four of von Marenzeller's specimens was attached to the inner side of a fragment of the same species. It is a member of Duncan's *Trochocyathoida* and because of the absence of pali belongs in a group with *Ceratotrochus.*

The genus *Anisopsammia* is proposed for the *Amphihelia* (subsequently, ? *Stereopsammia*) *rostrata* of Pourtalès. This genus is separated from *Coenopsammia* by having the calices on one side of the corallum, facing one way. I consider the genus of very doubtful value, being inclined to the opinion of Pourtalès that his *Dendrophyllia profunda* and ? *Stereopsammia rostrata* are congeneric.

One of von Marenzeller's generic names must be changed. Professor Verrill* has shown that *Madrepora oculata* Linn. = *Amphelia* or *Amphihelia oculata* of nearly all authors from Milne Edwards and Haime to the present time, must become the type species of the Linnaean *Madrepora.* This is an extremely inconvenient change, but unless we throw over entirely our rules of nomenclature it must be made.

The validity of a few of the species is doubtful.

The five plates that illustrate the paper are very good.

This is an excellent paper and the criticisms are of a minor nature. It supplements the work that Alcock has been doing on the deep-sea fauna of the Indian Ocean and on that around the Dutch East Indies. I have practically completed a report on the deep-sea corals dredged by the United States Fish Commission around the Hawaiian Islands and the illustrations are far advanced. The Hawaiian fauna, although it contains quite a number of new things, bears very considerable resemblance to that of the East Indies and the Indian Ocean. The Fish Commission collections will make an important addition to the Indo-Pacific faunas.

There are in our United States National Museum considerable collections of deep-sea corals dredged off the western coast of America. These should be studied in connection with the general subject of Indo-Pacific deep-sea faunas, to which Moseley, Alcock and von Marenzeller have already made contributions of so much value. It is my hope that before a great while I shall be able to present a suitable report on these portions of our unstudied collections.

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WASHINGTON, D. C.,
October 19, 1904.

* *Trans. Conn. Acad. Sci.*, Vol. XI., pp. 110–113, 1902.

SOCIETIES AND ACADEMIES.

NEW YORK ACADEMY OF SCIENCES, SECTION OF BIOLOGY.

THE meeting of October 10 was devoted to reports on summer work by members. In the absence of Professor Underwood, Professor Sumner was elected temporary chairman. The members reported as follows: Professor E. B. Wilson worked at the Naples, Sorbonne and Roscoff laboratories, continuing his studies of germinal localization in mollusks. Professor Bashford Dean attended the zoological congress at Berne and the British Association meeting in Cambridge, and later visited places of scientific interest in France. Professor Bristol worked at the Bermuda Biological Station, of which he was one of the directors. Mr. Yatsu worked at the Tufts College laboratory in Maine. Mr. Kellicott worked at the Cedar Point laboratory, in Ohio, completing his studies of the development of the vascular system of *Ceratodus*. Dr. Dublin continued his studies of germ-cells at the Cold Springs Harbor laboratory. Dr. Townsend superintended the remodeling of the water-supply apparatus at the New York Aquarium. Mr. Bigelow conducted special courses for teachers in the summer school of Columbia University. Professor Sumner directed the laboratories and the biological surveys of the Bureau of Fisheries at Woods Hole.

M. A. BIGELOW,
Secretary.

DISCUSSION AND CORRESPONDENCE.

THE EARLIEST MENTION OF FOSSIL FISHES.

FOR the satisfaction of those interested in verifying a disputed reference of high antiquity, one, too, which possesses the distinction of including the first mention of fossil fishes in literature, it may be profitable to indicate the source of Dr. Emmons's statement in regard to 'Origines,' briefly noticed in two articles that have appeared in SCIENCE (Nos. 502, 508) under the caption of 'Variæ Auctoritatis.'

Dr. Emmons has been good enough to inform the writer that the authority upon which he relied for the remarks in question was the abridged translation of von Zittel's 'History of Geology and Paleontology.' The German edition of this work briefly summarizes the opinions of Xenophanes on fossils '*as reported by Origen*,' the learned third-century theologian. But here endeth not the first lesson, since there is no doubt that the lamented paleontologist was mistaken as to the author who has preserved for us the views of the enlightened Eleatic.

None of the writings of Origen contains the fragment of Xenophanes which treats of the nature of fossils, but we must turn for it instead to the '*Philosophumena*' (or 'Refutation of all Heresies,' i., 14) of Hippolytus, Bishop of Portus, a voluminous third century writer, and the first great scholar of the Roman church.* The rescue of this valuable work from oblivion through the discovery of a medieval manuscript at Mt. Athos, its publication at Oxford in 1851 under the guise of a continuation of Origen, and subsequent determination of its true authorship, constitute an interesting chapter in the history of paleography.

The scientific fragments of Xenophanes and various pre-Socratic authors have been conveniently brought together, with copious annotations, in the works of Hermann Diels,† Paul Tannery,‡ Mullach and others, and are briefly discussed in the first volume of Zeller's 'Philosophy of the Greeks.' None of these fragments, however, can compensate the loss of the historical compendium drawn up by Theophrastus, which contained abstracts of the scientific views in vogue prior to the Alexandrian age, and is known to us (save

* Concerning Origen and Hippolytus one may consult Schaff's 'History of the Christian Church,' Vol. II. (New York, 1883). Their writings have been extensively published in this country and abroad.

† 'Doxograhi Græci' (Berlin, 1879). *Idem*, 'Die Fragmente der Vor-Sokratiker, Griechisch und Deutsch' (Berlin, 1903).

‡ 'Pour l'Histoire de la Science Hellène, de Thalès à Empédocle' (Paris, 1887). The appendix contains a translation of the important treatise by Theophrastus 'On the Sensations.' Additional references are given in the bibliography of Osborn's excellent conspectus, 'From the Greeks to Darwin' (New York, 1894).

for the 'Sensations') only through later compilations.

At the risk of appearing tedious in a matter which is by no means of trivial importance, namely, that of accuracy of citation, we may direct attention to the following pertinent comments taken from the introduction to Tannery's work above referred to:

Combien de fois voit-on de nos jours des érudits, et des plus consciencieux, citer telle page et telle ligne d'un volume qu'ils n'ont jamais eu entre leurs mains! C'est la conséquence forcée du système de citations à la mode, et qui, indispensable pour certains ouvrages, n'en est pas moins inutile et, par suite, abusif la plupart du temps. Il y a là un étalage d'érudition aussi facile qu'illusoire; qui s'est donné la peine de vérifier, par exemple, cent citations de suite dans tel ouvrage moderne, même des plus justement renommés, peut savoir seul combien il a chance d'en trouver d'inexactes ou de complètement fausses sur cent autres au hasard.

Then follow these two excellent prescriptions:

(1) Ne jamais citer avec précision un travail sans l'avoir lu intégralement; (2) se borner à l'indispensable, c'est-à-dire aux seuls cas où l'on peut désirer que le lecteur, pour être mieux convaincu, ait effectivement recours à l'ouvrage invoqué.

C. R. Eastman.

Harvard University.

POSTSCRIPT.

Through the courtesy of the Editor of Science it is possible to append a word to the foregoing after proof had already been passed for press. In the issue of Science for October 21, Dr. Emmons suggests that remarks of mine with reference to him in an earlier article (No. 502) 'seem to flavor of a certain disingenuousness.' This may be construed as implying that the article was contributed subsequently to an exchange of personal letters, in which Dr. Emmons courteously disclosed his authority.

In point of fact this assumption, or implied assumption, is incorrect, my former article having been already in train of publication at the time our correspondence took place. Before it was actually printed there was opportunity to be sure, for certain alterations to be made in the proof, incorporating the later information obtained from Dr. Emmons. But having in the meantime taken pains to verify *his* authority, and finding that von Zittel too was in error, it seemed best to leave the original text unchanged, and bring up the matter afresh on some future occasion; and had this intention been carried out earlier, it is probable that our esteemed Washington colleague would have no grounds for misapprehension.

C. R. E.

October 22, 1904.

PALÆONTOLOGIA UNIVERSALIS.

The writer desires to call the attention of American geologists to the fact that this very important work has but 21 subscribers in the United States, while France has 63 and Germany 96. Certainly the geologists and geological libraries of this country are not yet supplied with this publication. Fasciculi I. and II. have been issued; these contain 97 sheets redescribing and refiguring 46 of the old and little known species. It is intended to issue annually from 150 to 160 sheets, treating of about 80 species. The annual subscription price is $8.00. Subscriptions may be sent to G. E. Stechert, No. 9 East Sixteenth Street, New York City. Those persons or institutions desiring further information regarding this work, with samples of the plates, will be supplied on application to Professor Charles Schuchert, Yale University Museum, New Haven, Conn.

Charles Schuchert.

A PROPOSED GEOGRAPHIC DICTIONARY.

The increase in interest and the clearer understanding of the origin of many topographic features have caused a great increase in the number of what may be called technical topographic terms during the past twenty years. Further travel and study along geographic lines in foreign countries have led to the gradual adoption by us of many foreign topographic terms for various forms of relief. This growing body of terms is at present scattered through a large mass of literature, usually inaccessible to most students. The present writer's experience has shown very clearly the need for some authoritative dictionary of topographic terms which shall bring together not only the less well known terms, but also those which are now in frequent but very loose use. To meet this need he began

four years ago to compile a dictionary which should in some way serve, not only to fill the wants just pointed out, but also as a guide to appropriate names for new forms. A further use for such a list is suggested by a foreign term used in a recent government publication, and also *in another sense* in a somewhat earlier private paper by another writer.

The undersigned now wishes to appeal to all those interested in the advancement of the study of geomorphology, to aid in the preparation of this dictionary—already over three hundred terms have been catalogued—by sending to him the following data for any topographic term, in any language, which may be met with in the course of study or reading:

1. The new term, and the inventor or first user of it in the given sense.
2. The etymology of the term, if possible.
3. The publication, volume, page and year, where first used.
4. The original definition, preferably *quoted*.
5. The cited examples of the form or combination of forms to which the inventor, or first user, applied it.

The above need not be written on catalogue cards, but preferably should be typewritten. The latter is not at all a prime requisite, however.

Of course the contributors to the work will be given full credit for the aid they render. Letters should be addressed to

Cleveland Abbe, Jr.
1441 Florida Avenue, N. W.,
Washington, D. C.

AMŒBÆ FOR THE LABORATORY.

To the Editor of Science: Just at this season many teachers of zoology are looking for Amœbæ for their students, and as I remember well the difficulty that is often experienced in securing them in considerable numbers and of good size, I venture to give a very simple method of obtaining them which I hit upon accidentally two years ago and have found highly satisfactory ever since; it is quite possible that this method is in use by others and it may be that it has been recorded, but if so it has escaped my notice. Two years ago while examining some insect eggs which were attached to the lily-pads on a pond on my summer place, I noticed numerous amœbæ. So I suggested to my laboratory assistant the following autumn that he get a considerable number of lily-pads and remove the slime which adheres to the under surface with a spatula and put it in a shallow glass aquarium containing water six or eight centimeters deep. This he did, placing the vessel near a window, and in a week or two the amœbæ were very large and abundant on the surface of the sediment at the bottom of the aquarium. We followed the same method this year with equally satisfactory results, so that I believe it to be as reliable as it is simple, and I would strongly recommend it to any one who has had trouble in securing this useful animal.

A. W. Weysse.
Boston University.

DO RHIZOPODS DIE A NATURAL DEATH?

To the Editor of Science: In various works on zoology and geology statements like the following are usually found, and, so far as I know, have never been questioned: '* * * in the oceans Globigerinæ live in countless numbers. Dying, their shells accumulate to form thick layers on the ocean bottom.'

We know that as a rule protozoa do not die a natural death, as that term is used in reference to higher animals. They subdivide and we have two protozoa, these subdivide and there are four, and so on to the end of time. The fact that Globigerinæ protect themselves with a shell which consists of a series of chambers does not prevent them from withdrawing from their shell for purposes of conjugation and reproduction somewhat as do the diatoms. I would be very much obliged if some reader of Science who has studied the habits of rhizopods would answer the question, given above, in the correspondence department of your journal.

L. C. Wooster.
Department of Biology and Geology,
State Normal School, Emporia, Kansas.

BODY TEMPERATURE.

To the Editor of Science: In Science for September 9, Mr. Woods Hutchinson requests references to articles dealing with body temperature. If he has not already seen the vol-

ume, he will be interested in 'Principles of Animal Nutrition,' by H. P. Armsby, director of the Pennsylvania State Agricultural Experiment Station, which discusses this and related topics and summarizes a large amount of interesting information. The question is also taken up by Dr. Armsby in 'The Isodynamic Replacement of Nutrients,' SCIENCE, N. S., 18 (1903), No. 459, pp. 481–487.

Some experiments which have to do with temperature during fever with especial relation to the influence of the abnormal body condition on metabolism are summarized in Bulletin No. 45 of the Office of Experiment Stations, entitled 'A Digest of Metabolism Experiments.'

The *Jahresbericht der Tier-Chemie* contains numerous titles and abstracts of articles which deal with the question under consideration. The Department of Agriculture library contains a set of this journal, which can undoubtedly be found also in a number of other public or university libraries.

C. F. LANGWORTHY.

OFFICE OF EXPERIMENT STATIONS,
DEPARTMENT OF AGRICULTURE.

MATHEMATICS AND METAPHYSICS.

ON reading the interesting lecture of Professor Josiah Royce on 'The Sciences of the Ideal,' and learning that all leveling and serial relations come from the same root, one is reminded of the computer of the coast survey, who decided that 8×8 is not exactly 64, but plus or minus a small quantity, according to the table of logarithms he used. If mathematics and metaphysics coalesce where shall we rest? Will our mathematicians become 'flabbier and flabbier'?

A. HALL.

October 10, 1904.

SPECIAL ARTICLES.

WHAT IS AN ELECTRIC CURRENT?*

THE question of the day which seems to appeal most strongly to the physicist is: What is taking place in a metallic conductor on the terminals of some electrical source?

* Abstract from an address on 'Present Problems in Physics,' at the Congress of Arts and Science.

Rowland's rotating disk showed that a positively charged body moving in a positive direction, and a negatively charged body moving in an opposite direction, produce the same electromagnetic effects in the surrounding field.

Rutherford's work in deflecting the electrons of a radioactive body is in entire harmony with Rowland's result. Positively charged masses of radiant matter are deflected in the opposite direction from negatively charged matter, when acted upon by a magnetic field, the masses are, of course, moving in the same direction. These charged particles of moving matter are, in effect, superposed, or perhaps juxtaposed electric currents moving in the same direction. If either the α or the β particles could be reversed in direction, then the magnetic field would deflect them in the same direction. They would then each create the same external magnetic effects. They would then represent superposed currents of opposite sign, moving or flowing in opposite directions.

All of this means that a positive current of electricity flowing in a positive direction is not a negative current of electricity flowing in a negative direction. These two currents involve the motion of masses of matter in opposite directions. Do these currents co-exist in the conducting wire? Is a direct-current dynamo pouring oppositely moving electrons into the opposite ends of the conductor? After a few thousand years of continuous use, may it become clogged and lose in part its conductive properties, acquiring perhaps meanwhile radioactive properties?

Wheatstone made a famous experiment on the discharge of a Leyden jar, which was thought very instructive in his day. But did his contemporaries really learn the lesson which that experiment teaches. The sparks at the two gaps nearest the terminals of the conductor were formed before the central spark appeared. Have we not here evidence that the positive and negative currents, moving in opposite directions, begin at the opposite terminals, and only become superposed after an appreciable time interval?

In a Geissler tube having a length of about

fifty feet, J. J. Thomson found the positive luminescence to travel in a direction opposite that of the cathode particles in the Crookes tube, with a velocity somewhat greater than half the velocity of light. If this involved an actual transfer of electrons by some hand to hand process, it did not involve matter which was concerned in producing spectrum lines, since no Doppler effect is shown in the positive luminescence (Spottiswoode and Moulton). It might be suggested that the positive luminescence was here produced by cathode rays which found their way around the bends of this long tube and finally struck the anode, but this seems very improbable, especially in view of the Wheatstone experiment.

It seems probable that the discharge from a Leyden jar, or from a Holtz machine, differs from that of a spark coil or from the current originating in an armature of a dynamo. If both positive and negative currents exist in these last-named circuits, they are superposed in their origin, where the E.M.F. is produced. In the Holtz machine, as in the Leyden jar discharge, the positive and negative charges are separately accumulated, and in this case the positive and negative currents may perhaps be prevented from being superposed in the same discharge conductor. One terminal of the machine or jar may be connected to a large many-pointed conductor, suspended in the outer air on silk fibers. That terminal of the machine is then grounded on the dust particles in the air. The other terminal may be grounded either in a like manner, or on the gas pipe. Wheatstone gaps may be made in either of these circuits of unipolar discharge. These gaps may be either in open air or in tubes having any degree of exhaustion. Wheatstone's experiment under these conditions is likely to tell us something about the motion of positive and negative electricity in a conductor.

It is interesting to observe here that a Crookes tube will operate well in either of these unipolar circuits. The same exposure to the X-ray will give pictures of equal density when developed together in the same bath. In one case, however, the cathode terminal is connected to the negative terminal of the machine. In the other circuit, it is connected to the air contact, and is acted upon only inductively by the anti-cathode which is connected to the positive terminal of the machine. In the first case the cathode discharge appears normal and stable. In the second case, it seems unsteady, it is greatly affected by the motion of surrounding bodies, and it can be suppressed entirely by bringing the open hands near the tube so as to partly enclose it.

The pointed air terminal may be replaced by a flaming gas torch made of gas-piping closed at its upper end and having many small perforations, the lower end being connected with the gas main by rubber and glass tubing, the latter being kept dry by heating. The carbon particles then serve as the carriers. An insulated water tank in open air, from which the water escapes in a fine spray, is also effective.

If what we call the free charges on the terminals of the machine are delivered to the small particles which float off into the air, it would seem that in these unipolar discharges we may find that even if positive and negative currents are superposed they may not be equal in value. For certainly in both circuits the dominating action comes from the machine.

Francis E. Nipher.

SCIENTIFIC NOTES AND NEWS.

Professor Simon Newcomb has been elected a corresponding member of the Vienna Academy of Sciences.

It is reported that the Nobel prize for medicine will this year be awarded to Dr. Robert Koch.

Professor William T. Sedgwick, head of the biological department of the Massachusetts Institute of Technology, has been elected an honorary member of the New England Water Works Association.

Professor Martens, director of the Institute for Testing Materials at Berlin, has been elected a member of the Berlin Academy of Sciences.

Professors Paul Mansion, professor of mathematics, at the University of Genth, M.

Laisant, of the Ecole Polytechnique, Paris, and Dr. Giuseppe Peano, professor of mathematics at the University of Turin, have been elected honorary members of the Physico-mathematical Society of Kasan.

PROFESSOR E. SALKOWSKI, chief of the Chemical Laboratory of the Pathological Institute of the University of Berlin, has been elected a foreign member of the Society of Sciences at Upsala.

DR. O. DRUDE, who has recently been in this country to attend the International Congress of Arts and Science, has celebrated his twenty-fifth jubilee as professor of botany at the Dresden Institute of Technology. Professor Drude is also director of the Botanical Garden at Dresden.

WE learn from *Nature* that the friends of Professor Carey Foster, F.R.S., are taking the occasion of his recent retirement from the principalship of University College, London, as an opportunity of showing their appreciation of him by promoting a fund with the object of having his portrait painted for presentation to the council of the college, and a replica for presentation to Mrs. Foster. The president of the movement is the Right Hon. Lord Reay, and the vice-presidents are Sir Norman Lockyer, Sir Oliver Lodge and Sir Arthur Rücker.

MR. N. C. HAMNER, a graduate of the University of Virginia in the class of 1902, and Mr. A. W. Clark, a graduate of the University of Vermont in the class of 1904, have been appointed assistant chemists in the Agricultural Experiment Station of the Pennsylvania State College. The U. S. Secretary of Agriculture has appointed Mr. F. W. Christensen assistant expert in animal nutrition, in conformity with the agreement recently made with the station, and assigned him to duty in connection with the cooperative experiments with the respiration calorimeter. Mr. R. E. Stallings has been transferred from the position of assistant chemist to that of assistant in animal nutrition, and during the remainder of the year will devote his entire time to the investigations with the respiration calorimeter.

AT a recent meeting of the board of trustees of the Ohio State University, James S. Hine, associate professor of zoology and entomology, was granted leave of absence for the winter term of the present college year. He will spend the time in Guatemala collecting zoological specimens for the museum of that institution.

SIR DYCE DUCKWORTH has been appointed medical referee to the British treasury and adviser to the pensions' board, *vice* Dr. Lionel Beale, F.R.S., resigned.

THE Gedge prize in physiology of Cambridge University, has been awarded to Mr. K. Lucas, fellow of Trinity, for his paper on 'The Augmentor and Depressor Effect of Tensions on the Activity of Skeletal Muscle.'

AN examination of Black River near Bessemer was made last summer by a Michigan geological survey party including Mr. W. C. Gordon and C. E. Smith. The latter has had to resign from the Louisiana Survey on account of malaria.

MR. DESMOND FITZGERALD, of Boston, has recently returned from Manila, where he was sent by the United States government to report on questions of sanitation.

MR. JOHN MORLEY made the address at the celebration of Founder's Day at the Carnegie Institute, Pittsburg.

A MEMORIAL meeting was held at Chicago on October 23 in honor of the late Dr. N. S. Davis. The South Park commissioners have named a park in his honor.

PROFESSOR CLEMENS A. WINCKLER, the eminent chemist, died at Dresden on October 8, at the age of sixty-six years.

PROFESSOR MAX BARTELS, of Berlin, known for his publications on ethnology, died on October 22, at the age of sixty-two years.

DR. K. S. LEMSTRÖM, professor of physics at Helsingfors, died on October 2.

THE U. S. Civil Service Commission invites attention to the examination for scientific aid in the Department of Agriculture, applications for which may be filed at any

time. Eligibles are desired at this time to fill a vacancy in the position of scientific aid (male), with a knowledge of agricultural statistics, in the Bureau of Statistics, at $480 per annum, and other similar vacancies as they may occur. For the vacancy mentioned, only such applications will be considered as are filed with the commission at Washington prior to the hour of closing business on November 30, 1904. Applicants must be graduates of colleges in courses of study tending to qualify them for the scientific work of the Department of Agriculture. They are not assembled for this examination, but must submit all the required material with their applications.

THE New York Historical Society has received a gift of about $200,000 toward its new building to be erected on Central Park West between Seventy-sixth and Seventy-seventh Street.

THE French government proposes to establish a museum of industrial hygiene.

MR. SEDDON, the premier of New Zealand, has introduced a bill to prevent the further exportation of Maori antiquities from that colony. The penalty is fixed at £100.

MYLIUS ERICKSEN'S expedition, after two years and a half exploration of Greenland, returned on November 6 to Copenhagen. It is said that valuable ethnological records have been collected, the explorers having lived with the natives, studying their language and customs.

THE College of Physicians of Philadelphia announces that the next award of the Alvarenga prize, amounting to about one hundred and eighty dollars, will be made on July 14, 1905. Essays may be on any subject in medicine, but can not have been published. They must be received by the secretary of the college, Dr. Thomas R. Neilson, on or before May 1, 1905.

THE South African Philosophical Society has asked for a charter under the name of the Royal Society of South Africa.

THE National Association of State University Presidents met last week at Des Moines, Ia.

THE twenty-second congress of the American Ornithologists' Union will convene in Cambridge, Mass., on Monday, November 28, at 8 P.M. The evening session will be devoted to the election of officers and the transaction of other routine business. The meetings, open to the public and devoted to the reading and discussion of scientific papers, will be held in the Nash lecture-room, University Museum, Oxford St., beginning on Tuesday, November 29, and continuing for three days. Information regarding the congress can be had by addressing the secretary, Mr. John H. Sage, Portland, Conn.

THE fourth Pan-American Medical Congress will meet in Panama during the first week in January. According to the New York *Medical Record*, the Panama government has appropriated $25,000 for the scientific sessions and the entertainments. The afternoons will be devoted to the scientific sessions and the mornings and evenings to trips and social functions. So far as can be learned, the program in Panama will be a reception on the first day by President Amador, and the formal opening session of the congress the same evening. On the second day, an excursion to the canal in the morning, meeting of the various sections in the afternoon, and a banquet in the evening. On the third day, an excursion down the bay to Taboga Island, where a Panama breakfast will be served, scientific sessions in the afternoon, and a ball in the evening. On the fourth day, an excursion to the U. S. army barracks in the morning, section meetings in the afternoon, and the formal closing session in the evening. On the fifth day, an excursion to the plantation of the United Fruit Company; and on the afternoon of this day, those who intend going to Cuba by way of Jamaica to attend the meeting of the Public Health Association, will sail for Kingston, while those who intend going by way of Vera Cruz, or returning home by way of New Orleans or New York, will remain until the following Tuesday.

THE School of Demonstration in Plant and Animal Breeding conducted at St. Louis un-

der the auspices of the American Association of Agricultural Colleges and Experiment Stations appears to have been a marked success. It was no little work to keep such a school in running order under the circumstances of a large shifting audience, but Professor J. H. Shepperd, of the North Dakota Agricultural College, who was in charge of this work, seems to have accomplished what it was intended the school should be, carrying out the full detail of the work as to preparation of buildings, equipments and the construction of a daily program. Speakers came from nearly all the experiment stations and colleges, and there was presented, before this audience of practical farmers and animal breeders, many of the newest and best ideas in regard to plant and animal breeding. During the last session, October 3–15, the demonstration work upon plant breeding was emphasized, showing that much work is being done at the experiment stations at the present time in developing broader methods. Results were divided into breeding cereals for resistance to drought and plant diseases. Professor Ten Eyck, of the Kansas Agricultural College, is making extended experiments upon drought resistance. Professor H. L. Bolley, of the North Dakota Agricultural Experiment Station, gave two lectures on breeding cereals for disease resistance. Assuming that the struggle for existence among plants and a survival of the fittest represents the principle which the breeder should use, Professor Bolley has for several years conducted plant breeding experiments eliminating by a harsh environment or the promotion of disease weak strains and types in a farm crop.

As we have already noted, Captain Robert S. Scott was expected to open the new session of the Royal Geographical Society on November 7 with an account of the British Antarctic expedition. We learn from the London *Times* that at subsequent meetings Lieutenant Royds will deal with the meteorology of the expedition, Mr. Ferrar with the geology, Dr. Wilson with the zoology, and Mr. Bernacchi with the terrestrial magnetism. Captain Scott has consented to tell the story of some of the leading incidents of the expedition to young people about Christmas, when there will be an abundance of lantern illustrations. At the second meeting of the session, on November 21, Dr. Hunter Workman will give an account of the explorations in the Western Himalaya recently accomplished by himself and his wife, Dr. Fanny Bullock Workman. At the next meeting, on December 12, Major Delmé Radcliffe will deal with the results of the Anglo-German Boundary Commission in East Africa, on which he was the principal British representative. After Christmas, Colonel L. Jackson, R.E., will give an account of another Anglo-German Boundary Commission, that which was recently at work in Nigeria. Another African paper will be that by Mr. B. H. Jesson, the surveyor who accompanied Mr. McMillan's expedition; he will deal with the portion of the Sobat basin in Abyssinia which has not hitherto been explored. Colonel P. H. M. Massy will give an account of his explorations in Asia Minor, which extended over several years. There will be two South American papers, one by Dr. H. Hoek on explorations in various parts of South America, while a paper by Mr. C. Reginald Enock will give an account of two recent journeys in outlying parts of Peru. It is hoped that during the session one of the leading members of the recent Tibetan mission will give an account of the geographical results which were obtained, and which are believed to be of considerable importance.

UNIVERSITY AND EDUCATIONAL NEWS.

THE alumni of the Massachusetts Institute of Technology are collecting a fund for current expenses, which now amounts to over $100,000 to be used in the course of the next five years.

HARVARD University has received from Miss Maria Whitney a gift of $5,000, to be known as the 'Josiah Dwight Whitney Fund,' the income of which is to be applied as a scholarship, not exceeding $200, or as two scholarships of $100 each, to aid meritorious students in the study of field geology or geography in the summer months, preferably in the mountain region of the western United States.

Any excess of the annual income may be applied at the discretion of the department of geology and geography in aiding meritorious students in making other excursions.

Dr. Isaac Roberts, the eminent astronomer, has bequeathed his residuary estate, probably over $150,000, to the University of Liverpool and the University Colleges of North and South Wales, for the purpose of founding scholarships, with preference to astronomy, biology, zoology, botany, chemistry, electricity, geology and physics.

Dr. John Grieve left $8,000 to the medical faculty of the University of Glasgow, which will be used to endow a lectureship in physiological chemistry.

William Mackenzie has presented to the University of Toronto the collection of fossils made by Dr. George E. Matthew, of St. John, N. B. The collection contains many type specimens.

Mr. Henry Evans, of Trinity College, Cambridge, has bequeathed to the university his collection of British Lepidoptera.

The following appointments have been made at the Massachusetts Institute of Technology: Messrs. Roy D. Mailey, George A. Abbott and Charles A. Kraus, research assistants in physical chemistry; Dr. Erastus G. Smith, research associate, and George C. Bunker, research assistant, in sanitary science.

At Oberlin College the following appointments have been made: W. D. Cairns, A.M. (Harvard, 1898), advanced from instructor to associate professor of mathematics; Miss Florence Fitch, Ph.D. (Berlin, 1903), to be associate professor of philosophy; Mr. C. H. Burr, A.B. (Oberlin, 1903), to be assistant in physics; Mr. W. H. Chapin, A.B. (Oberlin, 1904), to be assistant in chemistry; Mr. J. R. Luckey, A.B. (Oberlin, 1904), to be assistant in mathematics and physics; Frederick Anderegg, professor of mathematics, has returned after a year's absence abroad.

A. J. Carlson, Ph.D. (Sanford, '03), instructor in physiology in the University of Pennsylvania, has been appointed associate in physiology in the University of Chicago.

At the University of Oklahoma the following are giving instruction for the first time: C. C. Major, M.E. (Cornell), in charge of engineering; C. E. Gabel, Ph.D. (Vienna), instructor in embryology and histology; E. G. Woodruff, A.M. (Nebraska), instructor in mineralogy; and H. C. Washburn, Ph.C. (Michigan), instructor in pharmacy.

It has been arranged that the installation of Lord Kelvin as chancellor of the University of Glasgow shall take place in the Bute Hall, on Tuesday, November 29.

At the annual meeting of the governors of Durham University College of Science, Newcastle-on-Tyne, the name of the college was changed to the Armstrong College of Science in the University of Durham. Sir Isambard Owen, vice-dean of the faculty of medicine in the University of London, was elected principal of the college in place of the late Dr. H. P. Gurney, who was killed while mountaineering in Switzerland in August.

Professor E. G. Coker, of McGill University, has been elected to the chair of civil engineering at the Finsbury Institute of Technology, London, in succession to Professor Dalby.

At King's College, London, Professor Caldecott will lecture on general psychology during first and second terms of coming session; Professor Halliburton, on histological psychology, during first term, and Dr. C. S. Myers, on experimental psychology (with demonstrations and laboratory work), during the second and third terms.

At Cambridge Mr. P. V. Bevan, M.A., fellow of Trinity College, has been appointed demonstrator of experimental physics in succession to Mr. Skinner, who has resigned. Mr. C. Chittock, B.A., of Trinity College, has been appointed assistant demonstrator in succession to Mr. Bevan.

M. H. Poincaré, professor of mathematical astronomy at the University of Paris, has been appointed professor of general astronomy in the Ecole Polytechnique.